Summer 8-15-2019

The Long-term Causes and Consequences of Conflict: A Micro-level Behavioral Perspective

Joan Barcelo
Washington University in St. Louis

WASHINGTON UNIVERSITY IN ST.LOUIS

Graduate School of Arts & Science
Department of Political Science

Dissertation Examination Committee:
James L. Gibson, Chair
Brian Crisp
Raymond Duch
Guillermo Rosas
Margit Tavits

The Long-term Causes and Consequences of Conflict: A Micro-level Behavioral Perspective

by
Joan Barceló

A dissertation presented to
The Graduate School
of Washington University in
partial fulfillment of the
requirements for the degree
of Doctor of Philosophy

August 2019
St. Louis, Missouri

Table of Contents

List of Figures

List of Tables

Acknowledgments

I would like to express the deepest appreciation to my committee chair Professor James L. Gibson, who has the attitude and the substance of a genius. Without his mentorship, this dissertation would not have been possible. I am deeply indebted to my adviser Guillermo Rosas for his fundamental role in my doctoral work. Guillermo provided me with all the support, patience, and encouragement that I needed throughout my graduate studies. I also thank my committee members, Margit Tavits, Raymond Duch and Brian Crisp, whose thoughtful comments and suggestions were invaluable.

Moreover, I thank everyone in the department in political science for their feedback and support at different stages of my graduate school. In particular, I appreciate the support of Michael Bechtel, Dawn Brancati, Sarah Brierley, Daniel Butler, David Carter, Justin Fox, Matt Gabel, Christopher Lucas, Jacob Montgomery, George Ofosu, Sunita Parikh, Jon Rogowski, Norman Schofield, Betsy Sinclair, and Steven Smith.

I would like to acknowledge my colleagues and advisors from my research stays at the Center of Experimental Social Science in the Nuffield College at the University of Oxford and the department of political science at the University of Mannheim for their wonderful feedback. Special thanks go to my institutional hosts, Raymond Duch and Nikolas Marinov.

Finally, I would like to go on to thank my fellow graduate students for their support and suggestions throughout this process. A special thanks go to my dearest office mates and good friends, Taishi and Mauricio, and my Demun neighbor and good friend, Elena Labzina, for being such a great support in sharing our stress in graduate school, deliberating over our ideas and papers, as well as providing happy distraction to rest my mind outside of research.

Joan Barceló

Washington University in Saint Louis

August 2019

This dissertation is dedicated to my wife, Mireia, who has been a fundamental source of support and encouragement in life and throughout the course of my years in graduate school. I also dedicate this thesis to my son, Jan, who I hope you will be motivated to seek knowledge. This thesis work is also dedicated to my parents, Joan and Roser, whose good examples have inspired me to work hard for the goals that I wanted to achieve.

ABSTRACT OF THE DISSERTATION

The Long-term Causes and Consequences of Conflict:

A Micro-level Behavioral Perspective

by

Joan Barceló

Doctor of Philosophy in Political Science

Washington University in St. Louis, 2019

Professor James L. Gibson, Chair

This dissertation explores the behavioral determinants and legacies of conflict from a socio-psychological perspective. The three papers that compose this dissertation investigate use tools of causal inference to explore, on the one hand, the socialization processes that influence leaders' decision to pursue war-like policies and, on the other hand, the longue durée legacy of wartime violence on preferences, as well as on people's proneness to being swayed by messages of collective threat during conflict.

The first paper of the dissertation examines the long-term determinants of interstate conflict by exploring the impact of leader's socialization processes. Based on the soft power theory of international experiences and the impressionable-years socialization hypothesis, I theorize that leaders with the experience of attending a university in a Western democratic country should be less likely than non-Western-educated leaders to initiate militarized interstate disputes. I test this proposition by employing a new dataset, building on Archigos and LEAD, that includes background attributes of more than 900 leaders from 147 non-Western countries between 1947 and 2001. The results support the hypothesis, even when accounting for leader selection, time-variant country and leader-level controls, other leaders' background characteristics, and country and year fixed effects. This finding lends credence to the soft

power thesis of institutions on international sojourners, and highlights the value of considering leaders' background and experiences in conflict research.

The second paper of the dissertation shifts the focus from the long-term behavioral causes of wartime violence to its consequences. Specifically, this chapter studies the long-term legacies of wartime violence by exploring its impact on civic engagement. My argument is that exposure to wartime violence transforms people's psychological makeup in a way that increases their long-term civic engagement. I test this proposition using evidence from the Vietnam War by exploiting the distance to the arbitrarily drawn border at the seventeenth parallel as an instrument for conflict intensity. The results show that individuals who lived during the war in a province that was heavily affected by the conflict are more likely to be engaged in civic organizations in 2001, 26 years after the end of the conflict. I further find that this effect occurs because war exposure increases people's participatory values, not because of postconflict development.

The third chapter of the dissertation investigates the past-present nexus in the reignition of conflicts in divided societies. To begin with, this chapter offers the first estimate of the effect of priming collective/group threat on actual electoral outcomes. This is done through a region-wide field experiment embedded in an organization's leafleting campaign in the 2017 Catalan regional elections in Spain. Unlike most field experiments that randomize at the level of individuals, randomization takes places at the level of the precinct, equivalent to a small village. The cluster randomization allows me to evaluate the effect of the treatment on actual voting behavior and, thus, alleviates concerns related to using self-reported behaviors. The results show that in precincts assigned to receive a group threat prime, the vote share of Catalan pro-secessionist political parties increased by between 1% and 1.7% compared to the no-increase of precincts that received a vote encouragement with either no prime of group threat or no prime at all.

Building on research suggesting that historical conflict strongly influences the formation of preferences across generations, I propose a resonance theory of persuasion by which communities more severely affected by historical political violence should be more likely to be influenced by primes of collective/group threat. To test this hypothesis, I combine the field experiment with a novel dataset of local-level historical wartime violence and state repression in Spain based on the geographic location and size of about 2,650 mass graves throughout Spain. In line with the resonance hypothesis, the results show that primes of collective threat are particularly effective in those communities that were more heavily affected by the Spanish Civil War and the ensuing repression, with an increase by between 3.6% and 5.8% in the support for Catalan pro-secessionist parties—compared to the increase of between 1% and 1.7% of precincts with an average exposure to the historical conflict. These findings have far-reaching implications for our understanding of how conflict history combined with rhetoric of group threat enhances secessionist mobilization.

Introduction

A core mission of humanity is to secure peace. After decades of collective efforts with this mission, violent conflict remains a fundamental issue facing the world today. New forms of insecurity such as large-scale civil unrest and terrorism are added to more conventional forms of domestic and interstate wars. The World Bank (2011) currently estimates that about 1,5 billion people live in regions that have been affected by violent conflict. As part of this mission, understanding the causes of insecurity and its legacies remain a core component in scholarly research agenda.

For the most part, institutional and structural causes and consequences of conflict (e.g., domestic politics, ethnic divisions, geographic features) (Fearon and Laitin, 2003; Reynal-Querol, 2002; Weeks, 2012) dominate the scholarly debate. This dissertation contributes to the understanding of the long-term causes and consequences of conflict from a socio-psychological and micro-level perspective with three major chapters.

On the one hand, the first paper examines the role of socio-psychological factors as a long-term determinant of leader's decision to go to war. Specifically, this study theorizes that a key socialization factor, education in a Western country when young, makes leaders acquire values of tolerance and conciliation and, in turn, look for nonconflict resolution of disagreements in their foreign policy. This argument is tested with data that combine events of interstate

disputes with the background information of 902 national leaders from 147 non-Western countries for the period 1947–2001.

I find that Western-educated leaders are indeed less likely to go to war compared to non-Western-educated leaders from similar countries. Results are robust to the inclusion of control variables, year fixed-effects and country-fixed effects. In addition, I implement various strategies to address threats to the causal validity of the main relationship, including checking the validity across different definitions of the West, sub-sampling on random transitions, adjusting for the impact of education alone, with a proxy variable of intelligence and socio-economic status of the leader, and for another proxy that captures a leader's transnational networks and transnational empathy. Finally, I also test whether endogenous beliefs could drive the main finding.

Overall, this paper provides an original answer to the long-standing question of why states decide to resort to the use of force in international relations. In particular, it shows that these decisions are partly shaped by leaders' experiences early in life and, therefore, the result of long-term psychological processes.

On the other hand, the second and third chapters shift the focus from an analysis of the long-term socio-psychological causes of conflict to its long-term socio-psychological consequences. Empirical contributions on the sociological consequences of war suggest that exposure to conflict violence may be associated with patterns of individual- and community-level mobilization with evidence from the aftermath of the civil conflict in Burundi (Voors et al., 2012), Nepal (Gilligan, Pasquale and Samii, 2014), Sierra Leone (Bellows and Miguel, 2006, 2009), and Uganda (De Luca and Verpoorten, 2015).

However, such studies are limited in their ability to fully address the relationship between violence exposure and civic engagement. First, much of this literature remain theoretically

and empirically agnostic about the pathways leading to behavioral changes. Second, outcomes are usually measured in the aftermath of war—usually within the first decade after the end of the violence—in the midst of lingering violence or reconstruction efforts, which could bias the results in favor of more prosocial effects. Third, the non-random geographical distribution of conflict violence hampers our ability to establish a causal relationship from conflict exposure to preferences. Fourth, most evidence has thus far been limited to civil wars—in which perpetrators are domestic actors—where increased engagement may be associated with heightened inter-group tensions (Bauer et al., 2016; Mironova and Whitt, 2016). Fifth, differences in outcomes across communities in postconflict settings may be due to the effect of war but also due to non-response bias (e.g., death, migration) (Gilligan, Pasquale and Samii, 2014). Finally, most projects have relied upon retrospective self-reported conflict victimization data, which may be subject to recall and non-response bias (Brück et al., 2016; Child and Nikolova, 2018).

The second paper of the dissertation offers both theoretical and empirical contributions to the current literature by improving upon these limitations in the same project. Theoretically, I suggest a mechanism that may partly account for the long-lasting legacy of political violence on individuals' participation in social organizations. I argue that exposure to civil war-related violence leads to a transformation in people's psychological makeup, which affects their long-term civic engagement. This process takes place via, at least, a change in individuals' value preferences in a way that they give more importance to participatory values such as the subjective importantce of freedom of speech and people's efficacy in how things are managed in their proximate community, the society, and the government.

Empirically, I use the case of the Vietnam War to overcome the empirical challenges. I identify the long-lasting effects of war exposure by exploiting the arbitrarily drawn border along the 17th parallel between North and South Vietnam in 1954. The research design is

complemented with two unique data sources on the Vietnamese conflict: (1) the geographic distribution of the U.S. military campaigns drawn from a dataset containing province-level bombing intensity; and, (2) survey data collected in 2001 by the World Value Survey, which include respondents' province of birth, province of residence just before the end of the war in 1975, and place of residence for the period of 1990-2001. The main finding of this paper is that those respondents who lived during the Vietnam War in a province that was heavily affected by the conflict are more likely to be engaged in civic organizations in 2001, 26 years after the end of the conflict. In addition, I provide consistent evidence that the plausible pathway for this effect include an increase in people's participatory values.

These findings are robust to different measurement decisions in the outcome variables, removing individuals' self-selection during the war, accounting for selection bias in the sample due to death and migration, and only using the sub-sample of respondents from North Vietnam, those who did not experience a regime change. In addition to the main findings, the richness of the data allows me to investigate the psychological mechanisms of these associations. While I find suggestive evidence for the role of an increase in participatory values; I can rule out alternative mechanisms such as changes in the sociological composition of communities due to unequal survival or migration, postwar state investments, unequal cross-regional development, and overall changes in the post-conflict context. Therefore, this paper concludes that a psychological pathway offers the most plausible explanations for the long-lasting effects of war-related violence on civic engagement. Even though conflict seems to positively influence participation in public life in the long-term, a major concern in conflict research is that communities that have been historically affected by violence seem to be more likely to fall into conflict again (Besley and Reynal-Querol, 2014; World Bank, 2011).

In this regard, the third paper provides theoretical insights and empirical evidence that shed light on the historical and contemporaneous mechanisms for the reignition of conflicts in

divided societies. Political elites often heighten people's sense of collective threat to garner political support.[1] Scholars have tried to establish whether these fear-based appeals are effective, yet their findings mixed and contested for their lack of internal and/or external validity (Horowitz and Klaus, 2018; Mutz, 2018; Selb and Munzert, 2018). Advancing this prior work, this chapter experimentally evaluates the potency of collective threat messages in bringing about real-world political mobilization (i.e., actual electoral outcomes).

Comparative politics scholars acknowledge that historical events leave long-lasting legacies on contemporary behaviors. One of the major mechanisms has to do with the shift in people's socialization processes (e.g., individual, family, peers, and local environments)(Acharya, Blackwell and Sen, 2016; Lupu and Peisakhin, 2017; Nunn and Wantchekon, 2011; Rozenas, Schutte and Zhukov, 2017). In this chapter, I also suggest a *resonance* hypothesis of political behavior by which political messages are more credible and vivid—hence more effective in shaping voting behavior— if they *resonate* with prior socialization processes.

Empirically, I study these questions through a unique clustered randomized controlled trial and novel historical data on the hotspots of the wartime violence during the 1936–1945 period in the ethno-nationalist political conflict in the Spanish region of Catalonia. First, I present the results of a precinct-level region-wide experiment embedded in a partisan non-governmental organization's campaign in the 2017 Catalan legislative elections using administrative records. Between December 18 and 19—two and three days before the elections—an estimated 37,000 leaflets were left across 98 villages, with a total population of 80,000 inhabitants. In total, 140 villages were assigned to one of three following groups: a treatment group (63 villages), a placebo group (35 villages), and a control group (42 villages). The majority of households within a municipality in the treatment group received a

[1] I define collective threat messages as fear-based appeals that highlight the potential of a hostile action on someone or a community by an external force.

collective threat leaflet, which encouraged them to vote in favor of Catalan pro-independence by reminding voters about a recent episode of police brutality by the Spanish police forces. The majority of households within the municipalities in the *placebo* leaflet received the same encouragement to vote in favor of Catalan pro-independence parties, but with no reference to any threatening event. Finally, villages in the *control group* did not receive any leaflet at all. This allows me to test whether collective threat messages influence the support for nationalist/secessionist parties.

Second, I evaluate the heterogeneity of the *collective threat treatment* across villages' historical experiences with the Spanish Civil War and the ensuing Francoist repression (1936–1945). Historical violence intensively, yet heterogeneously, affected Catalonia. During and after the war, the Francoist regime and supporters often employed individual or mass extra-judicial executions against dissidents—leftist supporters and regional nationalists—and most were buried in nearby mass graves. Thus, I use the geographic information and the estimated size of about 2,650 mass graves scattered throughout Spain to construct a local-level indicator of the intensity of the wartime violence. Then, I test the resonance model by evaluating the interaction effect of the collective threat treatment from the field experiment with the indicator of historical violence.

My findings are based on official election outcomes at the level of the village. On the one hand, I find it increased Catalan pro-secession parties' vote share in villages allocated to the *collective threat* leaflet by between 1% and 1.7% compared to similar villages assigned to the placebo and control groups. The estimate corrects for imperfect compliance in the delivery company, and are precisely estimated. Moreover, the results also suggest that the vote shares of Catalan pro-independence parties rise at the expense of the vote shares of neutral parties—but not unionist parties. This means that not only threat messages increase support for nationalist/secessionist parties, but also fuel local-level political polarization.

On the other hand, I find a significant increased effect of the *collective threat treatment* among those Catalan communities that were more severely impacted by the Spanish Civil War and the Francoist repression. Specifically, while the treatment effect on the support for Catalan pro-secessionist political forces means an increase of between 1.4% and 2.4% in those villages with an average history of wartime violence and state repression, the support for nationalist/separatist political forces increased by between 3.6% and 5.8% in those villages that are one standard deviation above the mean in the value of historical exposure to violence. These findings have far-reaching implications for our understanding of how conflict history combined with the contemporary rhetoric of collective threat may enhance nationalist-secessionist mobilization and political polarization.

In summary, the three chapters of this dissertation reflect the importance of incorporating micro-level behavioral analysis in order to advance our current understanding of political violence processes. While institutional and structural factors have been dominant in the study of war and conflict for decades, each chapter shows the importance of individuals' socialization experience from a different perspective. On the one hand, the first chapter of this dissertation shows the significant long-term impact of early socialization (i.e., university education) on leaders' decision to engage in militarized interstate disputes. On the other hand, the second chapter shows the long-term influence of early-life experience with political violence on social attitudes (i.e., participatory values and civic engagement). Similarly, the third chapter shows how collective threat messages are more influential among communities more severely affected by historical conflicts; that is, in those communities in which threat messages might resonate more with their prior experiences.

Chapter 1

Are Western-Educated Leaders Less Prone to Initiate Militarized Disputes?

Abstract

Recent theories on the causes of war focus on how institutional and structural factors shape leaders' decisions in foreign policy. However, citizens, policy-makers, and a growing number scholars argue that leaders' background experiences may matter for both domestic and foreign policy choices. This paper contributes to an emerging body of scholarship on leaders in international relations by showing how personal attributes influence the initiation of militarized disputes. Based on the *soft power theory* of international experiences and the *impressionable-years hypothesis* of socialization, I theorize that leaders with the experience of attending a university in a Western democratic country should be less likely than non-Western-educated leaders to initiate militarized interstate disputes. I test this proposition by employing a new data set, building on Archigos and LEAD, that includes background attributes of more than 900 leaders from 147 non-Western countries between 1947–2001. The results strongly support the hypothesis, even when accounting for leader selection, time-variant country and leader-level controls, other leaders' background characteristics, and country and year fixed effects. This finding lends credence to the soft power thesis of academic institutions on international sojourners, and highlights the value of considering leaders' experiences in analyses about international relations.

A country's involvement in militarized disputes between countries has largely been explained by impersonal forces, institutional and structural factors. However, much is yet to be learned about how the backgrounds of decision-makers may influence the way nations behave. Just as for ordinary citizens, early-life experiences and major life events are likely to leave a long-lasting imprint on leaders' future personality traits, attitudes and behavioral tendencies, and thereby shape the decisions they make. Among these experiences, international mobility is a prevalent life event that especially affects university students. In particular, an international educational experience exposes sojourners to an internationally diverse social environment and, in most cases, to a completely new system of values and beliefs. Thus, the large number of non-Western would-be national leaders studying in Western democratic countries should make us wonder about the impact of this experience on their future foreign policy-making. This paper begins to answer this broader question by looking at a specific aspect of leaders' background: are Western-educated leaders less likely to initiate militarized interstate disputes?

Anecdotal evidence yields mixed results. On the one hand, Gandhi represents a paradigmatic case of a non-Western leading figure who received his academic training in the West and then transformed his country by applying the values of freedom, democracy, and non-violence. Another noteworthy case is Aung San Suu Kyi who, after graduating from Oxford, returned to Burma to begin an active political career advocating for human rights and peace. On the other hand, Pol Pot's first involvements with Communism occurred in Paris where he joined the French Communist Party while studying at the Engineering School of Information and Digital Technologies. After four years of Western education, Pol Pot returned to his country to become the leading figure of the Khmer Rouge in the genocide of up to one-quarter of Cambodia's population.[2]

[2]Although the exact figure remains contested.

There has not been any systematic attempt to examine whether Western-educated leaders are more or less likely to initiate militarized interstate disputes. This neglect is largely caused by a broader scholarly reluctance to incorporate leaders' characteristics into models of political and economic outcomes (Jervis, 2013). In reaction to the standard pure institutionalist approach, some scholars have begun to look into leaders' characteristics when evaluating how institutions affect leaders, and how leaders affect outcomes. For example, Horowitz, Stam, and Ellis (2015) empirically tested whether leaders' education influenced a country's likelihood to engage in interstate military disputes and found no effect. Further, Gift and Krcmaric (2016) argue that education alone is not sufficient for instilling the values of freedom and democracy to a leader, but that Western education is. They find that countries whose national leaders have a Western educational background are more likely to democratize. Therefore, the lack of evidence for the education effect reported by Horowitz, Stam, and Ellis (2015) might be because the values that foster international peace are brought about not by any education, but specifically Western democratic education.

This study theorizes that leaders educated in Western democracies are more likely to have been exposed to values of tolerance and conciliation, which, in turn, leads them to look for non-conflict resolution of disagreements in their foreign policy. The argument is tested with data that combines events of interstate disputes with the background information of 902 national leaders from 147 non-Western countries for the period 1947–2001. I find that Western-educated leaders are indeed less likely to go to war compared to non-Western-educated leaders from similar countries. Results are robust to the inclusion of control variables, year fixed-effects, and country-fixed effects. In addition, I implement various strategies to address threats to the causal validity of the main relationship, including checking the validity across different definitions of the West, sub-sampling on random transitions, adjusting for the impact of education alone, with a proxy variable of intelligence and socioeconomic status

of the leader, and for another proxy that captures a leader's transnational networks and transnational empathy. Finally, I also test whether endogenous beliefs could drive the main finding.

The results of this study contribute to the emerging literature showing that elites with different background characteristics tend to have different views and pursue different policies with respect to a wide range of domestic issues, such as gender-related policy outcomes (Berkman and O'Connor, 1993; Swers, 2002) or economic policies (Carnes, 2012; Carnes and Lupu, 2015; Griffin and Anewalt-Remsburg, 2013), by extending it to the area of foreign policy. While some scholars had already shown the effect of prior military or combat experience on foreign policy (Horowitz and Stam, 2014), this study examines the effect of non-military prior experience on foreign policy. In addition, this paper adds new evidence to the recent debate about whether educated leaders are better leaders[3] by using a more nuanced measure of education that is closer to the theoretical mechanism.[4] Furthermore, this article provides an original answer to the long-standing question of why states decide to resort to the use of force in international relations, by showing that these decisions are partly shaped by leaders' prior experiences in life. Whether Western-educated leaders are linked to less aggressive military policy has direct implications for countries' national security. In particular, Western democratic countries might be more effective in achieving their political aims by wielding their non-military power, such as attracting prospective international leaders to their universities, as part of their national security strategy.

[3]See, for instance, Besley, Montalvo and Reynal-Querol (2011) and Jones, Olken et al. (2005).
[4]This follows a suggestion in Carnes and Lupu (2016).

1.1 The Politics of Western-Educated Leaders

The idea that leaders are essential to explaining political events is not a modern one. In fact, historians have long embraced that political leaders are responsible for shaping the history of politics. The nineteenth-century idea of history is probably best expressed by Thomas Carlyle's famous dictum that "the history of the world is but the biography of great men" (Carlyle, 1841). However, the "great men hypothesis" largely overlooks the role of institutions in shaping leaders' incentives, and it fast lost traction with social scientists. Since the advent of modern quantitative social sciences, the exploration of the sources of interstate disputes has centered on impersonal forces. This practice derives from two phenomenon: institutionalist dominance and hopelessly unsystematic subjects (Jervis, 2013). One the one hand, there was the belief that individuals do not matter, or matter little, in explaining major political outcomes (Waltz, 2010). Instead, scholars stressed the importance of incentives and constraints, including institutional dynamics, domestic politics, ethnic divisions, and major international events, to explain leaders' decisions (e.g., Croco and Weeks, 2016; Prorok, 2016; Weeks, 2012). On the other hand, although some admit that leaders may have played a role on some occasions, individuals' decisions are believed to be too rooted in individualistic causes. In other words, leaders' characteristics, so the argument goes, are too unique to lend themselves to broad generalizations and manageable theories.[5]

Though institutions surely exert powerful effects on the actors within them, individual leaders may have, at a minimum, some degree of leeway in their decision-making process. Reasonably, leaders have autonomy to shape their symbolic, legislative and executive functions, influencing the content and timing of enacted legislation, policies implemented, the statements made to their citizens, and the like. In reaction to the traditional institutionalist perspective, the

[5] For a detailed review of these arguments, see Jervis (2013).

study of political leaders is re-emerging at a fast pace. While the "great men" hypothesis is no longer part of the scholarly discussion, the long-lasting idea that variation in personal traits of influential individuals may impact political outcomes is increasingly seen as deserving of more systematic empirical examination.

The growing interest in leaders moves in two directions. On the one hand, some scholars emphasize how structural conditions influence leaders' preferences (Croco and Weeks, 2016; Chiozza and Goemans, 2011; Prorok, 2016). On the other hand, and most relevant for this research, scholars have recently shown that leaders' background characteristics influence policy-making and political outcomes in both domestic and foreign policy. Recent work on domestic policies has found that legislators' class background accurately predicts their policy preferences and legislative behavior (Carnes and Lupu, 2015; Hakhverdian, 2015; Lupu, 2015). In addition, national leaders' education is related to economic growth (Besley, Montalvo and Reynal-Querol, 2011; Jones, Olken et al., 2005), and pro-democratic institutional reforms (Gift and Krcmaric, 2016).[6] Similarly, researchers have also linked the prior backgrounds of political leaders to political outcomes. Revolutionary leaders with prior non-combat military experience are more likely to engage in military interstate disputes (Colgan, 2013; Horowitz and Stam, 2014; Horowitz, Stam and Ellis, 2015), and those with rebel experience are more prone to nuclear proliferation (Fuhrmann and Horowitz, 2015). Contributing to this small but emerging literature, I argue that Western-educated leaders are less likely to initiate interstate warfare. In the remainder of this section, I develop the rationale for my hypothesis by explicitly stating the what—Western democratic values—and the how—the channels of elite socialization in the West.

[6]However, see Carnes and Lupu (2016) for a challenge on the relationship between leaders' education and economic and political performance.

1.1.1 Western Values and Non-Violent Conflict Resolution

At the forefront of the studies on war and peace stands the democratic peace theory – the absence of wars between democracies. This empirical regularity has been linked to both an institutional and a normative causal logic. While the institutional mechanism contends that political leaders are re-election seekers who are beholden to internal interest groups and citizens (Bausch, 2015; De Mesquita et al., 1999; Fearon, 1994; Tomz and Weeks, 2013), the normative approach focuses on the role of the within-system dissemination of values (Maoz and Russett, 1993; Weart, 1998).

Central to the normative mechanism of the democratic peace proposition is that democratic systems socialize their political elites in values and norms in order to have them act according to its norms of behavior. The components of this value system include: the non-violent resolution of disputes, conflicts, and disagreements; learning to tolerate, negotiate and accommodate diverse views; and, the belief in the norms of "live and let live" as quintessential in a democratic political realm (Maoz and Russett, 1993; Weart, 1998). The values learned and applied domestically within democratic systems are externalized in norms of behavior toward other societies, which lead to more peaceful international relations, especially with other equally democratic societies.

An important aspect to consider is the geographic limits of the normative channel. There are three reasons to believe that the normative channel of the democratic peace thesis is plausible mostly within the boundaries of Western countries. First, although the normative mechanism blends all democratic countries into the same system of values, whether Western or not, the democratic peace thesis is not a universal empirical regularity. Henderson tested the main prediction of the thesis by splitting the sample into five world regions (Henderson, 2009). Revealingly, he found that while democracy is negatively linked to war in the sub-sample of

Western countries, the effect of democracy is either non-significant or positive in every single region outside the West.[7] Moreover, Dafoe (2011) also shows that the thesis is empirically supported only if regional dummies are excluded. Therefore, the democratic peace proposition may be best characterized as Western democratic peace, or simply as the Western peace proposition.

Consistent with this view, Inglehart, Welzel, and their colleagues have an important number of contributions in which they depict a two-dimensional structure of world values (Inglehart, 1997; Welzel, 2013*a*). In one of these dimensions, world societies are distinguished for their self-expression values—or emancipative values—as opposed to survival values. Emancipative values "give societies a more humanitarian, civic, democratic, and ecological outlook, and people who live in these societies are characterized by, among other things, a greater acceptance of individual differences in nationality or sexual orientations, the value of tolerance, and participation in public life. Importantly for the aim of this paper, Welzel and Deutsch argue that societies with a high level of emancipative values, which involve stronger humanitarian ideals, are less likely to engage in activities that may risk humans' physical integrity (Welzel and Deutsch, 2012; Welzel, 2013*a*). Because militarized disputes involve an obvious risk for the integrity of human beings, and especially their most extreme case of engagement in interstate war, individuals and societies with these values should strive for the non-violent resolution of disputes.

While any society around the world could, in theory, have high levels of self-expression or emancipative values, an empirical regularity is associated with Inglehart and Welzel's value map of the world: the system of values and beliefs that characterizes English-speaking world and Western European countries lies significantly above the world's average in the emancipative dimension of societal values (Inglehart, 1997; Welzel and Inglehart, 2008;

[7] For a similar point, see Goldsmith (2006).

Welzel, 2013*a*).[8] The nearly perfect empirical collinearity between Western societies and societies with long-lasting democratic political institutions does not allow researchers to disentangle the Western component from the democratic component of societies' system of values.[9] As a consequence, I argue that Western societies, which tend to have well-established democratic systems, are likely to socialize their political elites through their institutions and social environments in values and norms that include the non-violent resolution of disputes, conflicts, and disagreements; learning to tolerate, negotiate and accommodate diverse views; and, the belief in the norms of "live and let live", which are quintessential in Western long-lasting democratic political realms.

The next section explains how a Western democratic system of values is more likely to be acquired through socialization processes by national leaders who have attended a Western democratic academic institutions.

[8]Though the rise of post-materialist values only takes place after the 1950s and 1960s in Western countries (Inglehart, 1977), it is reasonable to believe that the prevalence of emancipative values in Western countries, the most affluent societies at least since the Industrial Revolution, has been greater than the emancipative values in most non-Western societies around the world, which have been less affluent in the modern history. In this regard, Inglehart argued that the emergence of post-materialist values are linked to the scarcity of material resources. In the absence of abundant material goods, people's survival values dominate over non-materialistic values. It is only once the satisfaction of the survival needs can be taken for granted that the focus will gradually shift to these 'non-material' goods. Thus, the differential in affluence between Western and non-Western societies justifies the idea that international students would have been subject to a cultural shock even before 1950s and 1960s.

[9]Whether the relationship between Western values and political institutions, so emancipative values and democracy, is correlational or causal remains debated by scholars. See Spaiser et al. (2014) for evidence against the existence of the effect of values on institutions and Welzel et al. (2017) for evidence on precisely this effect. However, the existence of such correlation is not debated. The relationship is empirically supported by the Polity IV democracy scores. While only four countries outside the English-speaking world and Western Europe have had a democracy score of 8 or greater since 1945 until 2011 (Costa Rica, Jamaica, Japan since 1952, and Trinidad and Tobago since 1960), only four Western countries have had a democracy score below 8 in one year or more since the end of the Second World War (France, Spain, Greece, and Portugal). Although I acknowledge that they are conceptually distinct in today's world, I will use the concept of Western and Western democratic countries as synonyms throughout this manuscript because of their historical collinearity.

1.1.2 Channels of Elite Socialization in the West

There are two major types of socialization mechanisms: strategic calculation and normative persuasion. The former stems from a rational-choice perspective, whereby actors are motivated by a logic of anticipated consequences rather than values or norms. In this regard, Waltz referred to the socialization effects of the structures in the international system as a process that "limits and molds behavior", but not beliefs (Waltz, 2010, 76). In this view, contexts and organizations constrain the behavior of the agents but do not change their internal characteristics. In contrast, I use a normative persuasion conceptualization, which defines socialization as a process of "inducting actors into the norms and rules of a given community" (Checkel, 2005, 804). Thus, institutions and social environments are causal variables with transformative effects on the basic characteristics of actors on how they see themselves, how they see their environment, and even what they think their interests are. In fact, the socialization mechanism that explains the relationship between Western-based education and foreign policy preferences can work only through the normative channel given the nature of the relationship between variables. When national leaders make decisions, they are no longer constrained by their country or institution of education and, thereby the logic of the mechanism cannot be explained by behavioral constraints, but by internalized policy preferences.

The idea that social communities such as countries or institutions socialize citizens, bureaucrats, workers, or students with the internalization of norms, values, ideas and even identities has a long tradition in the social sciences. At a basic level, values, norms, and ideas can spread through inter-personal contact (Allport, 1954; Pettigrew and Tropp, 2006), or through personal and online social networks (e.g., Bond et al., 2012). Besides peer contact, institutions can also exert a direct effect of mass-level political values, attitudes, and behaviors

(Christensen, 2015; Robbins, 2012), which may be then diffused through international mobility or migration (Pérez-Armendáriz and Crow, 2009). At the level of elites' socialization, a number of scholars have also shown how national representatives working in the European Commission and agencies of the European Union internalize a set of norm-guided rules and principled beliefs in collectively legitimating arguments, making decisions that reflect the views of their new organization, and expand their self-concept to include their organizational identity (Lewis, 2005; Quaglia, De Francesco and Radaelli, 2008; Suvarierol, Busuioc and Groenleer, 2013).

Socialization within a well-functioning Western democracy may cause many foreigners to learn and adopt the values, competences, and allegiances of Western democratic citizenship (Diamond, 1994). While there are multiple channels that may socialize foreign leaders, one of the most important ones is through the soft power of the schooling and university systems. The term "soft power" was coined to define the ability to impact the predilections of others through appeals generated by a process of admiration and emulation, rather than coercion (Nye, 2004, 2008). In this regard, one of the most effective tools to exercise a country's soft power is by hosting foreign students in university institutions. In the case of international students, Atkinson shows that not only those international students go abroad to acquire university education in a Western democratic country are more exposed to the liberal norms of their host institutions, but they also get a first-hand experience about the way of life in a Western country and its institutional functioning, which would likely influence the values that these students bring home after their educational experiences (Atkinson, 2010). In this regard, Spilimbergo (2009) reports that countries who have a greater proportion of students who have attended a university in a democratic country—the vast majority located in a Western country—are more likely to democratize.[10]

[10]Even though this investigation is framed as involving universities in democratic countries, the vast majority of foreign students attend Western universities, especially from the United States, United Kingdom,

Altogether, educational institutions seem to have an important effect on socializing their students in the values of the host country. The precise channels of these transformative effects can be found both inside and outside the academic institution. Inside the university, non-Western international students are exposed to distinct curricula and teaching styles. It is generally assumed that academic institutions reflect the societal values in their organization and management, and that students are exposed to curriculum experiences that develop in them a respect for country ideals. Thus, Western countries—with a stronger historical tendency for democratic regimes – and non-Western countries—historically more prone to authoritarian regimes – are not only different in their institutional and value systems, but also in the content of the curricula in the classes and the institutional organization of schools and universities (Finkel and Ernst, 2005; Kagan, 1991; Ma, 2007; Redish and Finnerty, 2002). In addition, Western universities regularly have a multitude of highly active organizations, including newspapers and magazines run by students, and extra-curricular activities, which, altogether, may bolster civic participation. In short, university institutions, by themselves, might impact would-be leaders' preferences by inducing them the Western democratic emancipative values characterized by a humanitarian view in which the non-conflict resolution of conflicts is thought to be a core component (Welzel and Inglehart, 2008; Welzel and Deutsch, 2012).

In addition to the impact of the in-class and on-campus life, the cultural impact for some non-Western international students with the off-campus lifestyle can also influence their long-term preferences. Living abroad infuses foreigners with the values of the place (Atkinson, 2010). In a Western democratic society, this means infusing their emancipative values and humanitarian worldview (Welzel and Inglehart, 2008; Welzel and Deutsch, 2012), having a

France, and Germany. In fact, the author acknowledges that Western democratic institutions spread modern values that other type of democracies do not when Spilimbergo (2009, 8) argues that "students who were educated at Patrice Lumumba University or who got religious education in Pakistan brought very different concepts of democracy to their home country than students educated in US or UK" (8).

first-hand experience with a institutional and value system in which citizens' participation is not only allowed but encouraged (e.g., uncensored political information, political activities, electoral campaigns, or politically-oriented social organizations), and domestic conflicts are solved peacefully. Therefore, we should expect leaders' preferences to be shaped by their on-campus and off-campus experience in a Western democratic country.

One of the primary reasons why university education is a significant transformative life experience is because it tends to occur at a crucial formative period for would-be leaders and citizens. The *impressionable-years hypothesis* maintains that core political attitudes, beliefs, and values crystallize during a period of attitudinal malleability at a relatively young age as individuals enter adulthood—15 to 30-year-old period—and remain fundamentally unaltered thereafter (Krosnick and Alwin, 1989; Jennings and Markus, 1984; Sears and Funk, 1999; Visser and Krosnick, 1998). Therefore, experiences during this period, during which individuals' windows for attitudinal changes is open, have long-lasting effects on individuals' beliefs throughout the rest of their lifetimes. This period coincides with an individuals' formative years in their last stage of secondary school and tertiary education – college and graduate school. Perhaps because it occurs at a crucial formative period of one's personality, value and political orientations and attitudes, international mobility has been found to have deep and long-term personal and social consequences among students, with changes in even the most stable attributes like their core personality traits (Stoeckel, 2016; Ying, 2002; Zimmermann and Neyer, 2013). In conjunction with the time period in which it usually occurs, international mobility is characterized by a transition in one's social relationships, which is arguably the first and foremost attribute of life transitioning events (Neyer and Lehnart, 2007).[11]

[11]From this perspective, life transitions common in adolescence and emerging adulthood are characterized by shifts in the structure of social relationships: leaving parental home, engaging in relationships, or having children, can all be understood through the prism of either disconnecting from previous social relationships,

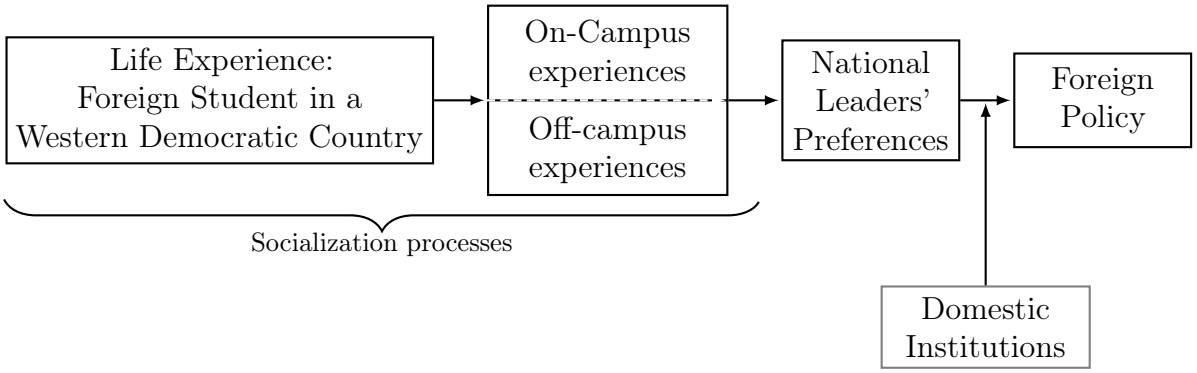
Even though I acknowledge that the theory of value transmission from Western institutions to visitors is applicable to broader experiences in the West during leaders' formative years, there are five reasons to focus on university education in the West. First, the major reason of international mobility to the West among non-Western young individuals is international education. Second, career-oriented goals such as the reputation of academic qualifications and degrees from Western academic institutions, and not learning the history, the values or enjoying from the political freedoms of Western countries, are the major force in the decision of non-Western students to attend a Western university (IIE/ACA, 2005).[12] Third, university education is easily detectable, leading to a measure of a strong international experience that is more reliable than other possible international experiences, such as work experience or private trips. Fourth, foreign education ensures a minimum amount of time of life in a Western country, compared to personal trips that can be as short as a few days. And, finally, not only education allows an individual to participate in the life in the host country, but also first-hand contact with the organizational characteristics of universities, a salient social institution.

Altogether, Figure 1.1 summarizes the theoretical framework used in this paper by depicting the theorized processes that go from the life experience of Western democratic-based education to foreign policy. In short, the steps are the following: 1) non-Western individuals travel abroad to acquire skills; 2) once abroad, they are exposed to distinct life experiences, both on-campus and off-campus; 3) their experiences set in motion a process of socialization that induces them to acquire the values of host country, hence, a Western democratic value system

the creation of new social relationships, or both (Hutteman et al., 2015; Kern, Della Porta and Friedman, 2014; Neyer et al., 2014).

[12]In a survey with 20,000 international students attending European universities, students reported that "career-oriented goals" was the major reason to choose their university. In a similar survey with international students in the U.S., 83% of the respondents reported that the "reputation of academic qualifications and degrees" was one of the major reasons to choose an American academic institution. Therefore, university education seems to be little affected by self-selection mechanisms.

Figure 1.1: The Theoretical Relationship between Life Experience as a Foreign University Student in a Western Democratic Country and Leaders' Foreign Policies



Note: This diagram shows the theoretical relationships between leaders' background and policy outcomes. For a more general conceptualization, see Horowitz and Stam (2014, 530).

that is characterized by emancipative values – a humanitarian worldview that incorporates a non-violent resolution of conflicts; 4) this process shapes leaders' preferences; and, finally, 5) conditional on domestic institutions, non-violent leaders' preferences decrease the likelihood to implement aggressive foreign policies. Even though I acknowledge the relevance of domestic institutions in shaping foreign policy, this paper focuses on the average treatment effect of changes in national leaders' preferences on their foreign policy. Even if domestic institutions impose constraints on leaders' decision-making, the expectation is that socialization in the West increases the likelihood of non-violent leaders' preferences and, in turn, makes war involvement less likely, all else equal. Thus, the details of the interaction between leaders' preferences and domestic institutions is beyond the scope of this manuscript. All together, the transformational effects of living in a Western democratic society as international students among non-Western would-be national leaders gives rise to the following empirical expectation:

Hypothesis 1: *Non-Western countries whose national leader was educated in a Western university leader are less likely to be involved in militarized interstate disputes.*

1.2 Research Design

This section overviews some relevant characteristics of the dataset, the measurement of the dependent, independent, and control variables, and the empirical strategy for the identification of a causal effect.

1.2.1 Sample of cases and unit of analysis

My data relies on the Archigos dataset to identify the universe of political leaders from 1875 to 2001, and the date of entering and exiting of office along with the reason for leaving office (Goemans, Gleditsch and Chiozza, 2009). In countries where there is more than one leader, Archigos uses what Gleditsch and Ward identified as the *effective* leader of each independent state (Gleditsch and Ward, 1999). In general terms, Archigos follows the simple rule of using the prime minister for parliamentarian systems, and the president for presidential, mixed systems, and non-democratic countries. Since the measure of Western education, as well as most covariates, is only available for the postwar period, I restrict my analysis to the period 1947–2001. The final sample consists of 902 leaders from 147 non-Western countries.[13]

There are several options available for selecting the unit of analysis. Some studies looking at the relationship between leaders' characteristics and democratization chose the leader-country as their unit of analysis (Besley, Montalvo and Reynal-Querol, 2011; Besley and Reynal-Querol, 2011; Gift and Krcmaric, 2016). Given that political institutions are very stable, this allows capturing the changes a leader has created throughout their term rather than changes in specific years. However, it artificially inflates the weight of short leader spells by counting all leaders equally, regardless of the number of years who governed. Other studies looking at the effect of leaders on interstate disputes have used leader-year as the

[13]See the online Appendix A for the full list of countries included in the main analysis.

unit of analysis because a dispute is a year-specific political event (Horowitz and Stam, 2014). Thus, I employed monadic tests that use leader-year as my basic unit of analysis with two exceptions. First, I add as many observations as active wars during the year if a country is involved in multiple militarized disputes in the same year. And, second, I follow a country year observation dataset—rather than leader-year—in those country years in which there are not militarized disputes. The implementation of these changes in the dataset are fully consistent with the decisions made in prior literature[14].

1.2.2 Dependent Variable

My main dependent variable of interest is the initiation of militarized dispute by a country's government. To build the variable, I employ data from the Militarized Inter-State Disputes (MID, version 4.1) provided by the Correlates of War (COW) dataset (Palmer et al., 2015). This dataset collects information for every conflict, defined as an instance in which one or several independent countries threaten, display, or use force against one or more states from 1816 to 2013. Thus, the main dependent variable measures whether a country is immersed in an interstate dispute initiated by the same country. Thus, each country i either is immersed in a militarized dispute in year t or not.[15] Accordingly, the dependent variable $MID_{i,t}$ has the following form:

$$MID_{i,t} = \begin{cases} 0 & \text{if country } i \text{ is not involved in an interstate war initiated by country } i \\ 1 & \text{if country } i \text{ is involved in an interstate war initiated by country } i \end{cases}$$

[14] See Horowitz et al. (2014) for the exact same procedures.

[15] To ensure results are not driven by inherited conflicts, I constructed a second dependent variable that captures only whether wars were initiated by country i during the period leader l was in power. See Section "Sensitivity to Coding Decisions" and Table A.1 in the online appendix B for the re-estimation of the main findings using this alternative dependent variable.

1.2.3 Independent Variable

To construct the key independent variable reflecting the educational experiences of world leaders, including whether a leader has had university education in a Western country, I expand upon Gift and Krcmaric's dataset by collecting biographical information of world leaders from a variety of sources, including encyclopedias (Gift and Krcmaric, 2016). In particular, I focus on coding whether a leader has university-level education and, if so, whether any of the institutions the leader attended for undergraduate or graduate studies was located in a Western country. While any definition of the West is disputable, I define a Western-educated leader as a leader who attended any university institution located in one of the following two sets of countries. First, Western European countries that did not belong to the Soviet block during the Cold War: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and the United Kingdom. Second, the block of countries that due to their shared cultural background with Western countries share similar emancipative values: Australia, Canada, New Zealand, and the United States. This latter set of countries are characterized by being both English-speaking countries and former British settlement colonies in which the British Empire replicated their home institutions to them; they also called Neo-Europes (Crosby, 1986).[16]

[16] An argument could be made against the inclusion of countries that were not continuously democratic in the post-Second World War era. To address this potential criticism, I checked the robustness of my results after excluding Spain, Portugal, and Greece from my definition of Western countries. In addition, I argued above that my mechanism works through democratic values disseminated from long-lived democratic countries, most of which happen to be Western countries. To make my findings robust not only for all Western countries but also to all long-lived democratic countries, I check their robustness after the inclusion of all countries that have been democratic, a polity IV democracy score greater than 8 during the entire period since the World War II. From the previous list, this definition excludes France, Greece, Spain and Portugal, and includes Costa Rica, Jamaica, Japan since 1952, and Trinidad and Tobago.

Figure 1.2: Percentage of Non-Western National Leaders Educated in a Western Democratic Country per Year

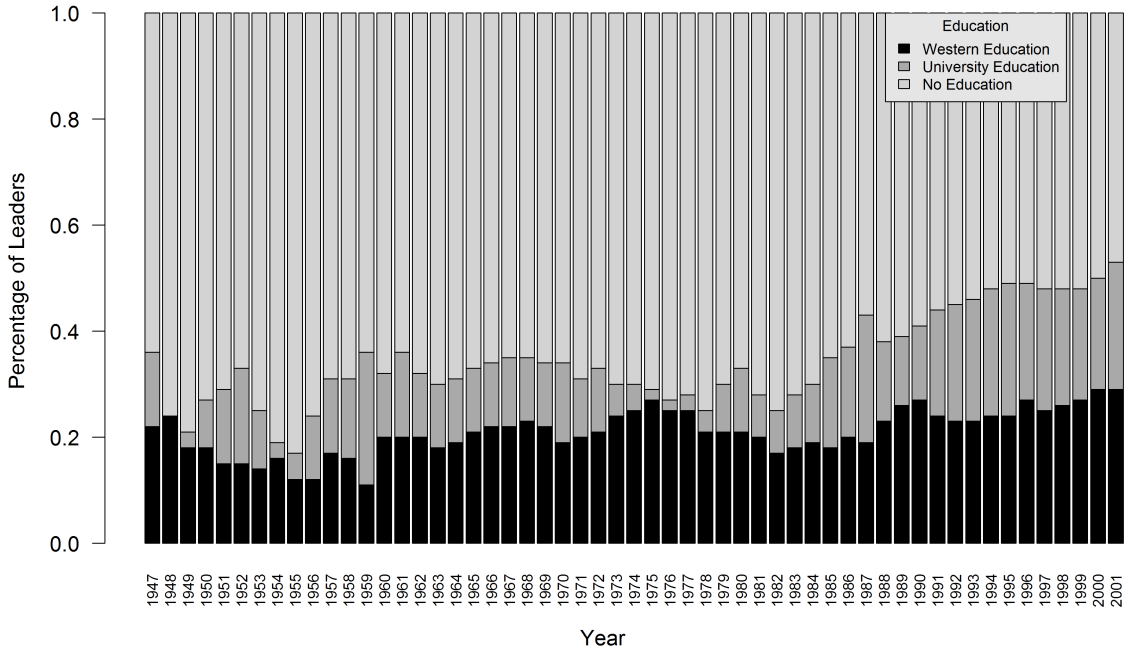
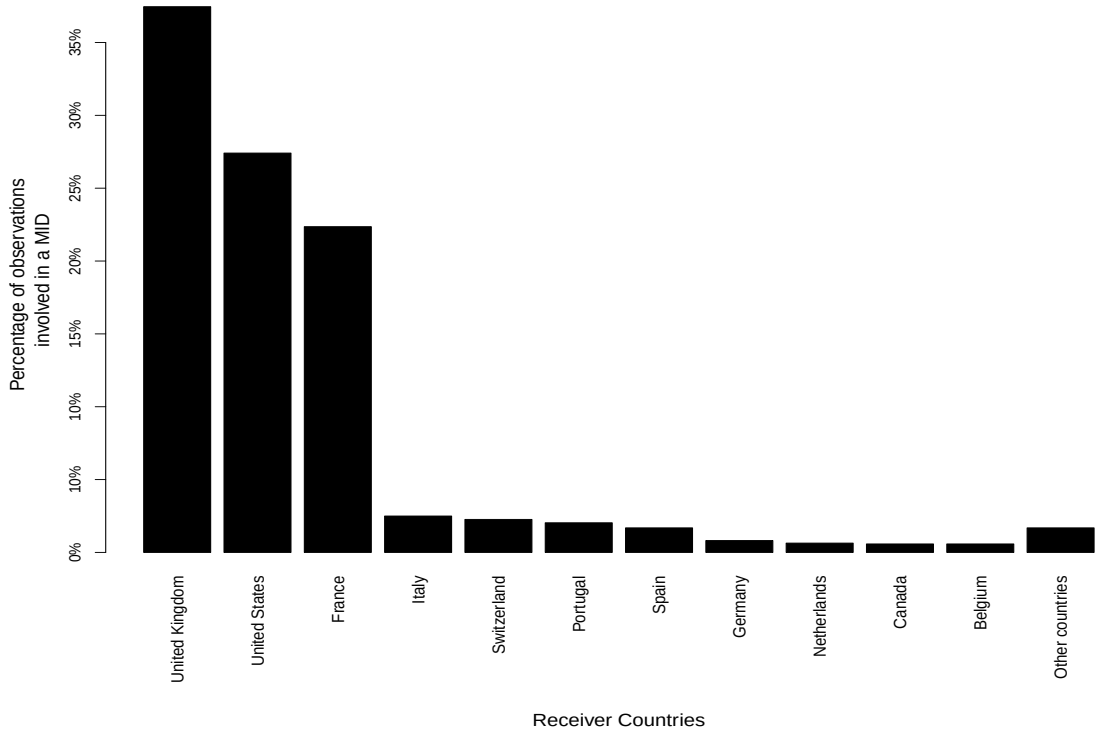


Figure 1.2 displays the variation over time in non-Western national leaders with Western tertiary education. The time trend in leaders with a Western tertiary education seems to have moved slightly upward, but the shift is not substantively significant and the percentage of Western-educated leaders is quite stable. The plot also show that most non-Western leaders do not have a Western educational background. As for Western democratic educated leaders, the years with the greatest percentage were 2000 and 2001, with 29% of the non-Western worldwide leaders having been educated in the West. On average, the percentage of non-Western national leaders with Western-based tertiary education is 15.7% throughout the entire period.

More generally, the percentage of non-Western national leaders with a university education has clearly been increasing over the second half of the twentieth century. While about only

Figure 1.3: The Distribution of Non-Western National Leaders Educated in a Western Democratic Educated by Country of Reception



one quarter of the national leaders during the period 1947–1988 had attended a university, with some variation within the range of 20-40% of the sample, there is an important increase of about 10-20% in leaders with university education during the period 1989-2001, ranging between 30-40% of the sample each year. This is the consequence of the entrance to the sample of those countries that originated after the collapse of the USSR and Yugoslavia. However, while many leaders in these new countries were educated, most of them had not been educated in the West. Therefore, the increase in the proportion of educated leaders outside the West does not translate into an increase in the proportion of non-Western Western-educated leaders.

Another interesting piece of information is to observe where most non-Western national leaders go to the university conditional on going to a Western democratic country. Figure

1.3 shows the distribution of leader-year observations with an educational experience in a Western democratic at the level of a university with respect to their host country. As we can observe, most leaders that go to the West attend a university in the United Kingdom (37.5%), the United States (27.4%), and France (22.4%), compared to any other Western country (12.7%).

1.2.4 Control Variables

In this section, I list the control variables that might confound the basic bivariate association between a leader's educational background and involvement in a militarized dispute. For this, I included several national-level control variables that are standard in the field of interstate disputes. For most of the variables, I take data from the year before the leader enters office to ensure controls are only included if they cannot be a consequence of the leader itself.[17] To minimize the issue of omitted variable bias, I include a number of leader-level control variables as well.

Regime Type. A country's regime type is regarded as a major determinant of the likelihood of being involved in an interstate war.[18] In addition, democratic countries are more likely to select more educated leaders (Besley and Reynal-Querol, 2011). Therefore, regime type is a major control of the empirical tests.

Economic Development. One of the most robust determinants of international conflict is economic development. To the same extent that upper middle and high-income countries rarely engage in any form of civil war (Fearon and Laitin, 2003; Hegre and Sambanis, 2006; Ward, Greenhill and Bakke, 2010), higher income countries are unlikely to engage in interstate

[17]To avoid losing information on leaders from countries entering in the dataset (e.g., new states), I use data from the first available year.

[18]For an extensive review, see Tomz and Weeks (2013).

disputes (Mousseau, 2000). Additionally, high-income countries might be more likely to send their leaders to study abroad. Therefore, this is also an important confounding variable that the empirical models should take into account. Thus, I include the country-year GDP per capita in the models.

National material capabilities. National material capabilities, or military power, can affect the likelihood of getting involved in interstate wars by increasing the likelihood of a war victory. This argument can go in two directions: a stronger country has more incentives to initiate an international military dispute due to the higher likelihood of success, but a country's strength can also decrease the likelihood of conflict due to the deterrence effect of its military power. Even though I don't have strong directional expectations about its association with the selection of Western-educated leaders, I follow similar prior work and include it to make the main findings robust to this variable (Horowitz and Stam, 2014; Horowitz, Stam and Ellis, 2015). To measure a country's national material capabilities, I rely on the widely-used Composite Index of National Capability (CINC) from the National Material Capabilities dataset (version 4.0) collected by the Correlates of War Project (Singer, 1988). The CINC is a composite score based on six indicators: military expenditure, military personnel, energy consumption, iron and steel production, urban population, and total population.

Past war experience. Similar to the national material capabilities, a defeat or a victory in a country's past war experience may impact the likelihood that a country engages in an interstate dispute in the future. Furthermore, the effect from a past defeat or victory can also trigger instability that may fuel internal tensions leading to an increased likelihood of civil war, with potential for its trans-nationalization. At the same time, the legacy of past wars can also affect the decision of potential leaders to study abroad or to stay in the country by means of limiting the available resources or changing potential leaders' need to strengthen their educational skills. Therefore, I include two controls in the model that are related to

past war experience. First, a variable indicating the *number of years since the last militarized interstate dispute*. Second, two control dummy variables that capture whether the *past war experience* ended up in victory or defeat: the excluded category is if the last war ended in a draw.

National student flows. The number of a country's young population studying abroad may affect both the likelihood that any Western-educated individual seized power in that country and the popular support for militarized interstate disputes. Therefore, the models are adjusted for a measure of a country's total number of students (in thousands) studying in the United States, using data from the Institute of International Education (2005).

Distance to the West. The distance to a Western country may be a determinant of Western-based education. In addition, proximity to Western countries, whose level of regime stability might be greater than non-Western countries, can also reduce the likelihood of engaging in conflicts. Therefore, I add a control that takes the minimum value of the distance between the centroid of the country and the centroid of the following countries: Australia, Austria, Finland, Germany, Greece, Italy, New Zealand, Spain, and the United States. These countries are selected because they represent, broadly speaking, the perimeter of the West. Distance is computed in kilometers using the C-shapes data in 2001.[19] Thus, the value for Argentina is 8,965, which corresponds to the distance to the United States, the closest of the above countries to Argentina; but, the value for Iraq is 2,026, which is the distance to Greece, the closest of the above countries.

Ethnic fractionalization. More ethnically or religiously diverse countries are more prone to conflict (Collier and Hoeffler, 2004; Esteban, Mayoral and Ray, 2012*b*,*a*; Reynal-Querol, 2002).

[19] To reduce computing time, I use a simplified version of the country polygons according to the Douglas-Peucker algorithm with a tolerance of 0.2. See Weidmann et al. (2010) and Weidmann & Gleditsch (2010) for further details.

At the same time, ethnic fractionalization can dampen development through a reduction of investment, which might indirectly influence investment in human capital, and also so the likelihood of potential leaders to get education abroad (Montalvo and Reynal-Querol, 2005). Even though this is perhaps a more influential factor for intrastate disputes rather than interstate disputes, I still control for it.

Colonial legacy. Many leaders from postcolonial countries had experienced their formative years during the colonial time. If the leader lived in a colony with a Western metropolis, this may have eased their way to Western academic institutions. If, instead, the leader had her formative years in an independent postcolonial country, this may still increase her likelihood to go to the West due to the colonial legacy that may facilitate the entire process (e.g., language proficiency, fellowships). In addition, the status of being a former colony surely affects the position of the independent country in the international arena. Using data from the Quality of Government dataset (Dahlberg et al., 2016), models are adjusted for the colonial legacy of the country with a dummy that captures whether the country was a former colony or not.

Prior Occupation. Leaders with a high socioeconomic status may be less likely to go to war because they have more to lose. And, also, they may be more likely to study abroad because their families can afford more costly education. Even though there is no data available on the socio-economic background of leaders, their occupation before entering in politics is a reasonably good proxy variable for their prior social class experiences, and widely used in comparative politics for similar purposes (Carnes, 2012; Carnes and Lupu, 2015; Hout, Brooks and Manza, 1995). Using the LEAD dataset (Horowitz, Stam and Ellis, 2015), I classify the prior occupations of national leaders into nine categories: businesspeople, blue-collar workers, career politicians and activists, lawyers, military personnel and police officers, religious figures, scientists and engineers, gentry and nobility (e.g., landowner, aristocrats, royalty),

and service-based professionals (e.g., teachers, journalists, doctors, interpreters). Thus, I adjust the main relationship for leaders' occupations prior to their political career. I include these categories as a set of dummies in the model.

Level of educational attainment. Beyond Western-education, education alone can also instill values that are relevant for a leader's future policy preferences. Even though this has already been tested by Horowitz et al. (2015), who found a null effect, I ensure that my results are not affected by the effect of education alone by adjusting my models for the level of educational attainment of the leader. To be clear, I do not expect that education does not matter, but that Western education provides an added effect for reducing war-proneness among leaders. Using the LEAD dataset, I add an ordinal variable that takes the following values: primary education (1), secondary education (2), a bachelor's university degree or equivalent (3), and a doctoral degree (4).

Non-Western Foreign Education. Besides values, Western education also brings other attributes such as transnational empathy or broader social networks (Gift and Krcmaric, 2016). On the one hand, studying abroad forces contact with new people and their new ways of life. Generally speaking, this may lead sojourners to humanize citizens from other places, develop empathy, and reduce animosity against people from other countries.[20] On the other hand, leaders with a foreign education may enjoy broader networks, which allows them to reap the benefits from these networks to solve confrontations through negotiation rather than conflict. Notice that these mechanisms are at work regardless of the country of study as long as the leader studies abroad. Therefore, I ensure that the main coefficient is not affected by these processes by adjusting for a dummy that captures whether a leader had attended a non-Western foreign university.

[20]See Pettigrew and Tropp (2006) for a review on *intergroup contact theory* and Haslam (2006) for a review on *humanization processes*. They constitute the theoretical basis of these statements.

Education in a top university. Compared to non-Western-educated leaders, Western-educated leaders may be better trained, have higher capabilities, competencies, or just more intelligence. This can be the case because either intelligent or highly competent leaders self-select into these institutions or Western universities provide a higher education quality to their students. Regardless of either of the mechanisms, I tackle both by constructing a control variable that captures whether the leader attended an elite university based on a Western democracy. While there is not a widely accepted measure of high quality in academic universities, I code the dummy as 1 if the university is included among the top-ten universities from the U.S. News Report on university rankings in 1983.[21] Added to the U.S. academic institutions, I also include as elite schools The University of Oxford and Cambridge University from the United Kingdom. Controlling for top university generates a problem of lack of common support with the main independent variable. In short, this variable can only take a value of 1 if Western education also takes the value of 1. To avoid this issue, Western education is coded as 1 only if the university of attendance is not a top academic institution.[22]

1.2.5 Estimating Leader Effects

The aim of this project is to estimate the contribution of a leader to a country's involvement to a militarized interstate dispute and how this relates to the leaders' Western democratic-based education. My model specifications follow a two-stage process. I estimate the effects of leaders from the following regression:

[21]This is the oldest ranking of academic institutions that is currently available. Since my leaders' dataset involves the period 1947–2001, the year 1983 is temporally close to the center of the period and probably a good proxy variable for the quality of academic institutions in the modern U.S. history. By order of position, universities ranked in the top ten in 1983 are: Stanford University, Harvard University, Yale University, Princeton University, University of California - Berkeley, University of Chicago, University of Michigan - Ann Arbor, Cornell University, University of Illinois - Urbana Champaign, Massachusetts Institute of Technology, and Dartmouth College. There are eleven universities in the list of top ten schools because the last two institutions are tied in their ranking.

[22]Excluding the top university control and including both top and non-top academic institutions in the definition of Western education does not alter any of the findings presented in the results section.

$$Pr(MID_{itl} = 1) = logit^{-1}(\beta_1 E_{itl} + \iota_l + \lambda_t + \theta_i + X_{it}\beta + Z_{it}\beta)$$

where MID_{itl} is the involvement in a militarized international dispute in country i at year t when leader l is in office. On the right-hand side of the equation, E_{itl} is a dummy that captures whether in country i at year t the national leader l who is in office has a Western democratic educational background, ι_t are leader-fixed effects, λ_t are year fixed-effects, θ_i are country-fixed effects, X_{it} stands for other time varying controls, and Z_{itl} stands for leader-level controls. The key variable in the model is E_{itl}. In a first step, testing the null hypothesis that leaders do not matter in MID is equivalent to testing whether $\iota_l = 0$ in a null model with fixed-effects only model, that is, a model without leader, country and year-level controls.[23] If the null hypothesis cannot be rejected, then further discussion on the effect of leader-level characteristics becomes meaningless. If there is evidence of a significant cross-leader variation in MID, then I apply equation 1 to test my hypothesis. Briefly, testing the null hypothesis that leaders' Western-based education does not matter in MID involvement is equivalent to testing whether $\beta_1 = 0$.

1.2.6 Threats to the Identification of a Causal Effect

There are some issues that arise when claiming causality on the basis of this model specification. The first issue has been mentioned above and has to do with the possible presence of time-variant confounders. If there are country-time specific effects that may correlate with the Western nature of the educational attainment of the leader and a country's MID, then the causal effect cannot be identified. To strengthen my claims of causality, the main model includes X_{it}, a plausible set of country-year factors that might confound the main relationship, and which is detailed above.

[23] In the terms of equation 1, the null model with fixed-effects only would be specified as follows: $Pr(MID_{itl} = 1) = logit^{-1}(\iota_l + \lambda_t + \theta_i)$.

The second source of concern is endogeneity due to unobservable characteristics of the leader that are correlated both with their experience of going to the West and with a peaceful foreign policy. While I admit this is an intractable concern, I employ two strategies to address this issue. First, I have provided some quantitative and qualitative suggestive evidence showing that leaders study abroad to acquire skills and a prestigious degree, rather than because of their democratic values. Further, the main model also includes X_{it}, a plausible set of a leader's characteristics that might confound the main relationship.[24]

The third source of concern is endogeneity due to either endogenous transition timing or endogenous leader selection. On the one hand, leadership transitions are not random shocks, but they may be determined by the advent of conflict. Adding to this, leaders are selected as a function of a context. Altogether, there is potential that countries may transition their leadership to select non-Western-educated leaders when they face the expectation of war in the near future. Even though this is plausible, it is also very unlikely. In most cases, leaders are selected for domestic rather than international reasons. While accounting for these endogeneity issues is surely fundamental in studies of leader effects on domestic outcomes, such as economic growth or change in political institutions, it is less central in studies of international outcomes.

However, if leaders are selected in anticipation of the near future, then a significant relationship between Western-educated leaders and MID from the main model specification may be the result of two distinct processes: a) Western-educated leaders are less likely to take their countries to MID, or; 2) countries that are likely to face MID in the near future undertake leader transition toward non-Western-educated leaders in anticipation of the conflict-prone

[24]Some scholars have preferred to conflate the two process of selection and socialization in their estimated effects of institutions on individuals' preferences such as Adolph (2013), Kitschelt and Rehm (2014), and Weeden and Grusky (2005). While this might be appropriate in some instances, this paper takes another approach by trying to alleviate the endogeneity concerns that arise from self-selection processes by both controlling and matching for leaders' prior occupational background.

scenario. Even though we would like to know what is the specific mechanism, whether the effect of the leader while in office or the selection of the leader by a country's selectorate, both pathways are in line with my basic theoretical postulate, that is, that Western-educated leaders are less likely to go to MID.

To systematically investigate this alternative mechanism, I address the timing of the leadership transition by employing a technique that was first developed in the economic literature by Jones and Olken (2005), and more recently applied to the studies of leader effects in both economics and political science (Besley, Montalvo and Reynal-Querol, 2011; Gift and Krcmaric, 2016; Horowitz, Stam and Ellis, 2015). This technique exploits the source of exogeneity that comes from transitions caused by leader deaths from natural causes. Thus, I empirically analyze my main finding only on the sample of leaders that arrive in the office after a natural death. In this manner, I can now be confident that the date of the leadership transition is unrelated to pre-existing circumstances or anticipation of conflict in the near future. Thus, this strategy ensures that transitions are randomly determined with respect to MID, attenuating the concerns of endogeneity in the leader selection process with regards to the prospect of MID.

However, analyzing those leaders that achieve office after a random transition is not sufficient if the new leader is imposed by a foreign power or seizes power in an irregular manner. In this case, while the transition is random, the selection of the next leader is not. To alleviate this additional concern, I implement the main empirical specification on the subsample of leaders that, besides replacing a naturally deceased leader, achieve office through regular channels, so according to the provisions, rules and norms of a country (e.g., the second in line replaces the President or prime minister).[25] This explicitly excludes those remaining

[25]I follow the classification of regular versus irregular from the Archigos dataset (Goemans, Gleditsch and Chiozza, 2009).

leaders that entered in the office through an irregular manner, such as a coup, or through direct imposition by another state.

1.3 Results

This article estimates the relationship between the Western educational background of a country's national leader and that country's engagement in a militarized interstate dispute. Specifically, I test the hypothesis that countries with Western-educated leaders are less likely to initiate militarized disputes against other countries.

1.3.1 Is There a Significant Cross-Leader Variation in the Number of Militarized Interstate Disputes?

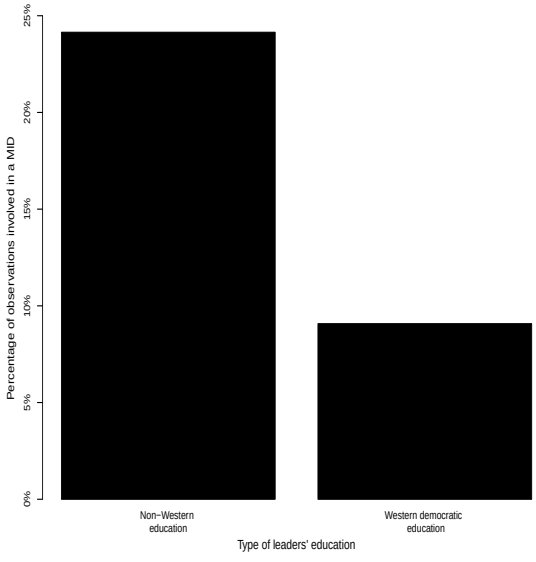
A first stage of the analysis should test whether there is significant cross-leader variation in involvement in militarized interstate disputes in a model where country and period effects are taken into account. In a null model of a mixed effects logistic regression with random intercepts by leaders, year, and country (not shown), the variance of the intercepts indicates that variation across leaders within countries is substantial. In particular, leader intercepts have a standard deviation of 1.21, compared to the variance of the year intercepts, whose standard deviation is 0.30, about one quarter of the standard deviation of leaders. Unsurprisingly, the cross-national differences are larger than the within-country cross-leader effects on bellicose disputes. In other words, knowing about a leader's country is more informative concerning the likelihood of conflict than knowing the leader itself, yet both of them are far more informative than just knowing the year the leader governed. Even though the country-level variance is greater than the leader-level variance, a great share of the within-country variance is attributed to the differences across leaders. Consequently, the

task of understanding what leader-level characteristics may influence the likelihood of the country initiating militarized international disputes is worth pursuing.

1.3.2 Main Findings

Before presenting the results from the multivariate regressions, Figure 1.4 shows the percentage of leader-year observations in the dataset that were involved in a MID during our period of analysis by the type of leaders' education. The plot shows that 24.1% of the leader-year observations whose national leader was not Western education were involved in a MID. By contrast, the percentage of observations with a MID decreases to 9.1% if the country has a national leader with Western-based education. Though the difference is substantial, I now proceed to implement the appropriate statistical analysis to improve the credibility for a causal claim between the type of leaders' education and countries' military foreign policy.

Figure 1.4: Percentage of leader-year observations involved in a MID conditional on the type of leaders' education



To begin with, Table 1.1 reports the results from a set of mixed effects logistic regression models that test the hypothesized relationship between Western-based education and war initiation across different model specifications, including leader, country and year random and fixed effects, as well as the introduction of country and leader controls. Because the data has a multilevel structure with leaders nested into countries, I implement a set of mixed effects models with random intercepts by leader to estimate the higher-level parameter of interest, whether a leader has had university education in a Western country.

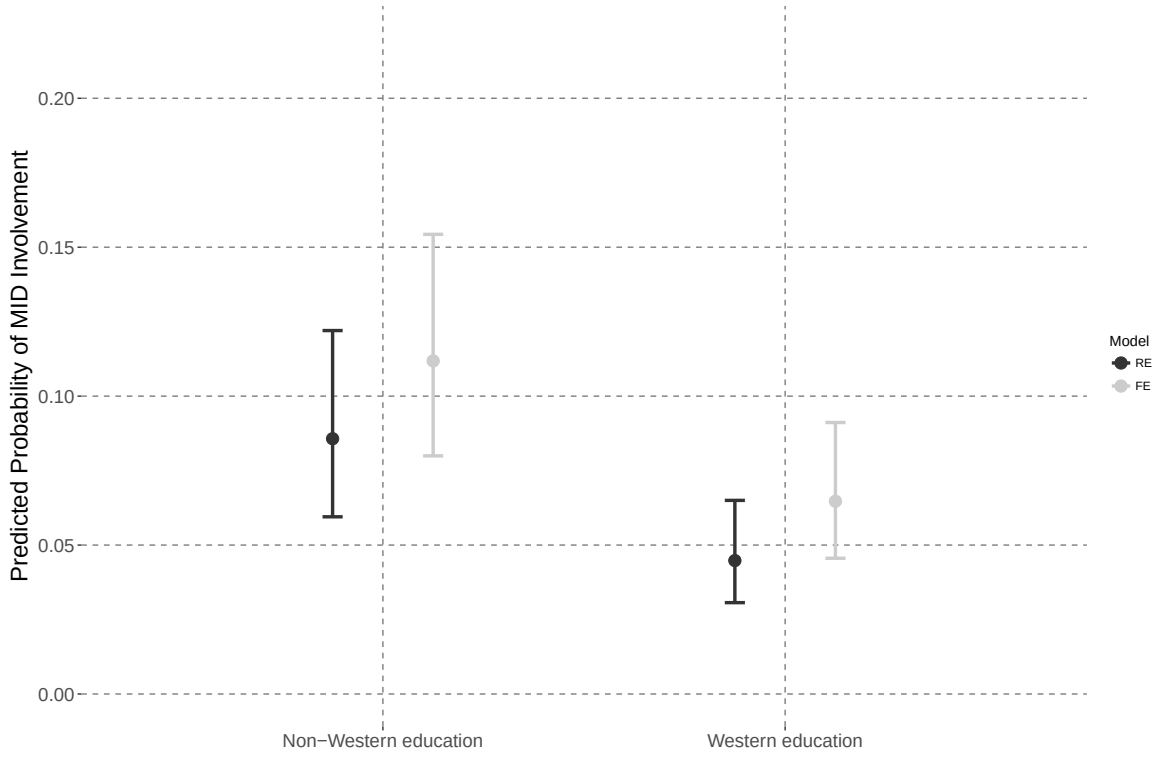
The effect of Western education on involvement in a war involvement is negative and statistically reliable at a 99% confidence level across all models. Model 1 reports the significantly negative bivariate relationship between Western education and war involvement, after incorporating a mixed effects models with random intercepts by leader, country, and year. Models 2 through 6 minimize the issue of omitted variable bias. Model 2 reports the main relationship after entirely removing any year-level variance, as well as partially-pooling the between-country variance. Model 3 adds adjustments to deal with endogeneity due to characteristics of the leader that may be correlated both with their experience of going to the West and with a peaceful foreign policy. This includes the level of studies of the leader and prior occupation of the leader before becoming involved in politics. In addition, Model 3 shows that leaders from top universities are not different from other Western-educated leaders in their propensity for war involvement, and that the simple experience of a foreign education (non-Western) does not suffice to reduce the likelihood of war involvement. By contrast, the effect of Western education on war involvement is largely unaltered after incorporating these adjustments. Models 4 and 5 estimate the country-level variance by adding country-level controls with and without leader-level controls. The main effects remains quite similar across these specifications.

Model 6 in Table 1.1 further deals with the omitted variable bias by entirely removing all country- and year-level heterogeneity. In other words, the model keeps the within-country and the within-year variance alone. Moreover, the leader-level controls, included in Models 3 and 5, and the country-level time-variant controls, included in Models 4 and 5, are maintained in this specification. This model requires the exclusion of countries that only have one leader, leaders that only govern for one year, as well as time-invariant country-level controls (ethnic fractionalization, colonial legacy, and distance to the West), which explains the reduction in the number of observations. Overall, while controlling for time-variant and time-invariant covariates reduces the effect of a leader's Western education, the remains significantly negative.

In order to illustrate the substantive significance of the effect of having a Western-educated leader, Figure 2 plots the simulation of the predicted probabilities as suggested by King et al. (2000). The simulated predicted probability of a country being involved in an interstate dispute that that country initiated in a particular year is expected to be 11.1% (from Model 5, Table 1.1) or 8.6% (from Model 6, Table 1.1) if a leader is not Western-educated.[26] This figure drastically shrinks if the leader has been Western-educated, with an associated probability of 6.5% (from Model 5, Table 1.1) or 4.5% (from Model 6, Table 1.1). In other words, the likelihood of being involved in an interstate dispute is reduced by about one-half as a function of whether the leader is Western-educated or not. The predicted probabilities are simulated at the mean values of the democracy score, the GDP per capita, student flow, national material capabilities, ethnic fractionalization (time-invariant variable, so not included in the FE model) and distance to the West (time-invariant variable, so not included in the FE model); they also assume that a country either had never faced a war or the last war ended up in a draw, does not have a colonial legacy (time-invariant variable, so not included in the FE model),

[26]Predicted values are calculated from a leader with domestic university education and all other values at the sample mean.

Figure 1.5: The Predicted Probability of International Disputes at Different Type of Leaders' Education (95% CI). Coefficients from Table 1.1, Models 5 and 6.



the leader holds a university degree, has not attended a top university nor a non-Western academic institution outside her home country, and she has had a lifelong political career.[27]

The controls in the models have the expected signs. Across all the models, an important predictor of interstate disputes is economic development. As expected, more economically developed countries are less likely to be involved in interstate disputes initiated by the same country. Since this variable does show substantive within-country variation over time, it remains statistically reliable even after including all other controls and adjusting for time-invariant characteristics of the country. In other words, variation across countries, as well as variation in the same country, affect the likelihood of involvement in disputes against other

[27]Simulations are based on the fully-specified model from Table 1.1 (columns 5 and 6). Setting different the values of the control variables does not change the substantive conclusions from the predicted probabilities.

countries. Another important variable is democracy. Democratic countries are less likely to initiate interstate disputes, yet even though the coefficient of democracy is consistently negative, they are only statistically significant at the 90% confidence level.

Beyond economic development and political institutions, the model confirms some of the security-specific confounders that are generally correlated with the initiation of interstate disputes in the prior literature, such as the outcome of the last war and material national capabilities. To begin with, we should take into account that the excluded category conflates countries that engaged in a war in the past that ended up in a draw with countries that have never engaged in a war. Thus, we should expect that the dummies for the outcomes in the last war capture the fact that a country engaged in a conflict, as well as the outcome of that conflict. As expected due to this coding procedure, countries that engaged in a dispute, regardless of the outcome of that dispute, are more likely to initiate another interstate dispute in the future. Similarly, results confirm the role of national material capabilities. More powerful states or, in other words, states with a greater capacity to exercise influence and resist external influence attempts, are more likely to initiate interstate disputes. The effect of the composite index of national material capabilities exerts a strong influence in boosting the likelihood of engaging in a dispute against another state. Variation across nations is particularly strong in this regard, with a confidence level of 99% even after adjusting for year-specific attributes. In the country fixed-effects model, however, the effect of national material capabilities becomes close to no-effect. The last time-variant control in the model is a confounder specific for my model since it specifically deals with the confounding effect of outbound internationally mobile students to the West. Even though there is no literature suggesting any expectation on its effect on interstate disputes, changes in the number of students going to Western countries is associated with more interstate disputes, although this effect is confounded by time-invariant characteristics of nations because we can see that

it become insignificant once we focus on within-country difference in international student flows over time.

Finally, there are three time-invariant characteristics of the country that may affect the likelihood of engaging in a dispute against other states: ethnic fractionalization, colonial legacy, and distance to the West. Of the three, colonial legacy is the predictor that consistently exerts a stronger effect on reducing the likelihood of engaging in a conflict—yet it is only significant at the 90% confidence level, but not at the standard level of 95% level. As expected, the effect of ethnic fractionalization is positive, with more fractionalized countries engaging in more international disputes, and the effect of the distance to the West is positive, although both effects are close to zero and do not reach any standard level of statistical significance.

Not only could the relationship between Western-based education of the leader and militarized disputes be spurious due to country-level characteristics, but it could also be a product of leader-level characteristics. Table 1.1 also reports models based on a number of leader-level adjustments. This set of models shows that those countries with leaders with no educational attainment are less likely to become involved in an interstate conflict compared to countries with leaders of higher educational background, although these difference are not significant. The lack of significant differences across levels of educational attainment is consistent with similar empirical findings in previous work (Horowitz, Stam and Ellis, 2015).

As expected from this paper's theoretical framework, the results also show that non-Western foreign education and Western education in a top university does not explain additional variance beyond the effect of studying in the West. Furthermore, the model also adjusts for a leaders' pre-office characteristics such as their occupational background before entering politics. Yet, these coefficients do not yield consistent results across the model specifications.

Table 1.1: Regressions of War Initiation and Western Education

	Dependent variable: Interstate Dispute Initiation					
	(1)	(2)	(3)	(4)	(5)	(6)
Western Education	−1.26***	−0.88***	−0.83***	−0.82***	−0.80***	−0.64**
	(0.24)	(0.20)	(0.22)	(0.20)	(0.20)	(0.20)
Leader Controls						
Secondary Studies			0.60		0.70	0.73
			(0.50)		(0.48)	(0.48)
Undergraduate			0.48		0.59	0.49
			(0.47)		(0.47)	(0.44)
Post-graduate			0.63		0.75	0.63
			(0.50)		(0.49)	(0.47)
Foreign education			−0.14		−0.10	−0.16
(non-Western)			(0.25)		(0.25)	(0.23)
Top University			0.08		0.19	0.45
			(0.34)		(0.33)	(0.31)
Prior occupation dummies?	N	N	Y	N	Y	Y
Country Controls						
Democracy Score				−0.03	−0.03	−0.02
				(0.01)	(0.01)	(0.01)
GDPpc				−0.39***	−0.38**	−0.43**
				(0.11)	(0.11)	(0.14)
Last War Won				0.66*	0.75*	0.80*
				(0.35)	(0.37)	(0.32)
Last War Lost				0.28	0.23	0.14
				(0.31)	(0.33)	(0.29)
Material Capabilities				21.8***	22.23***	−2.38
				(6.03)	(6.05)	(8.41)
Student Flow (000')				0.31***	0.10***	0.04
				(0.09)	(0.03)	(0.03)
Ethnic Fractionalization				0.04	0.13	
				(0.13)	(0.21)	
Colonial legacy				−0.57	−0.52	
				(0.32)	(0.34)	
Distance to the West (000')				0.02	0.01	
				(0.14)	(0.07)	
Random/Fixed Effects						
Leader RE	Y	Y	Y	Y	Y	Y
Year RE/FE	N	FE	FE	FE	FE	FE
Country RE/FE	N	RE	RE	RE	RE	FE
Constant	−2.24***	−3.29***	−3.27***	−1.32***	−1.23***	−0.41
	(0.12)	(0.58)	(0.70)	(0.93)	(0.74)	(1.27)
N	6,209	6,209	6,209	6,209	6,209	5,258
N Countries	147	147	147	147	147	111
N Years	55	55	55	55	55	55
N Leaders	902	902	902	902	902	768
LL	−2.386	−2,240	−2,221	−2,224	−2,221	−2,043
AIC	4,778	4,596	4,631	4,583	4,602	4,459
BIC	4,789	4,986	5,109	5,034	5,140	5,680

Note: *p<0.05; **p<0.01; ***p<0.001. Models are mixed effects logistic regression models with random intercepts by leaders, and random or fixed effects by year and country, depending on the specification. GPDpc, ethnic fractionalization and student flow are included in their logarithmic scale since this is the most appropriate functional form in the relationship between these variables and the outcome variable.

1.3.3 Robustness Checks

In this section, I check the robustness of the finding by re-estimating the models with different coding decisions of the independent and the dependent variables, an alternative matching procedure in which leader-year observations are matched according to observables characteristics and, finally, I deal with the endogeneity of leader selection by examining the deaths-in-office sub-sample.

Sensitivity to Coding Decisions

To ensure that results are not sensitive to some coding decisions, I have re-estimated the models using alternative definitions of the main independent and dependent variables. On the one hand, I have included in the definition of the West, all countries that, according to a Polity IV score of 8 or above, are democracies for the most part of the years in the sample (Costa Rica, Jamaica, Japan, and Trinidad and Tobago). I have also excluded those countries that have not been democracies in one year or more during this period (France, Greece, Spain and Portugal). In addition, I have re-estimated the model after re-coding the dependent variable to take the value of 1 if a leader becomes involved in an international dispute in that year and it was initiated by that leader, so excluding inherited conflicts, and 0 if otherwise. In both cases, the results remain substantively unchanged (see the online Appendix B and C, respectively).

Selection Bias: A Matching Approach

The research design establishes the effect of international education in countries' foreign policies whose leaders have been educated in a Western democracy ("the treatment group") and countries whose leaders have not been educated in a Western democracy ("the control

group"), yet both groups of leaders are drawn from the same country, period, and adjusted values in the other controls. A common issue with this type of regression-adjustments analysis is the lack common support. To check the robustness of the above model, I implement a matching technique based on leader- and country-level observables to increase the confidence that I am effectively comparing only comparable observations. In practice, I use a matching procedure to discard some data – unmatched data points – so that regression models fit better. To do so, I first match observations on all covariates used in Models 1-5 in Table 1.1 using the nearest neighbor matching technique. Then, I trim the dataset to include only matched observations ($1,355$ treated and $1,355$ control observations).[28]

The bottom panel in Table 1.2 reports the balance of covariates between the treatment and the control groups ensured with the matchning technique, except for the year dummies, which are included in the online Appendix due to space limitations. The difference in the means across all covariates are relatively small compared to the standard deviation of the variables in the control group. This indicates that the the treated and the control units are effectively comparable across all observables. A difference in means between the two groups shows that the proportion of observations with a militarized interstate dispute among non-Western-educated leaders is 13.8%, which is significantly higher than the proportion of 8.7% among Western-educated leaders.[29]

To further test the robustness of this effect, Table 1.2 reports a set of regression models after adjusting for the small imbalances in the covariates and including cluster-robust standard error by leader to correct for the within-leader correlation of observations. The OLS estimate reported in Model 1 shows that the adjusted difference in means is 5%, which is consistent

[28]I also implement a hybrid matching technique in which leader-year observations are matched to their nearest neighbor with regards to continuous variables and exactly matched on categorical variables. The results are substantively identical. See Table A.5 in the online Appendix.

[29]The difference in means is 5.1 with a standard error of 0.012, t-statistic = 4.21, p-value < 0.001.

Table 1.2: The Effect of Western Education on War Initiation: A Matching Approach

	Dependent variable: Interstate Dispute Initiation			
	OLS	*Logistic Regression Models*		
Western education	-0.05^{***}	-0.56^{***}	-0.64^{***}	-0.69^{***}
	(0.01)	(0.20)	(0.20)	(0.21)
Intercept	0.14	-0.23	-1.04	4.76
	(0.08)	(0.66)	(1.31)	(8.75)
Controls	Y	Y	Y	Y
Year FE	N	N	Y	Y
Country FE	N	N	N	Y
N Treatment Group		1,355		948
N Control Group		1,355		935
N Total		2,710		1,883

Balance of Covariates	**Treatment Group**	**Control Group**	**Control Group**	**Treatment Group**
	Means/proportions	Means/proportions	Standard deviation	Diff. in means/proportions
Level of Education	2.48	2.38	0.721	0.102
Businesspeople	0.107	0.124	0.330	0.017
Gentry	0.178	0.165	0.372	0.013
Blue-collar worker	0.080	0.103	0.305	-0.024
Military	0.152	0.177	0.382	-0.025
Lawyers	0.236	0.236	0.424	0.000
Religious	0.056	0.035	0.183	0.022
Scientists	0.082	0.082	0.274	0.000
Service	0.334	0.309	0.462	0.026
Democracy Score	-0.150	0.070	6.91	-0.228
GDPpc	7.04	7.08	1.37	-0.038
Last War Won	0.021	0.029	0.170	-0.008
Last War Lost	0.042	0.038	0.192	0.004
Material Capabilities	0.0014	0.0015	0.0023	-0.0001
Student Flow (000')	-0.192	-0.189	3.16	-0.003
Ethnic Fractionalization	0.488	0.474	0.275	0.014
Colonial Legacy	0.848	0.804	0.397	0.044
Distance to the West	3,978	4,046	2,001	-67.35

Note: *p<0.05; **p<0.01; ***p<0.001. Models report cluster-robust standard error by leader to correct for the within-leader correlation of observations. The matching procedure is nearest neighbor. See the online Appendix for a full report of the balance of the covariates in the unmatched dataset compared to the balance of the covariates in the matched dataset. Just as for all the models in Table 1.1, only non-Western leaders are used for the analysis. Controls "Y" means that all controls that are shown in the list of Balance of Covariates are also included in the regression models. Because the matching is not exact, the inclusion of the controls in the post-matching analysis adjusts for the remaining differences in the value of the covariates between the treated and the control units. Yet, the decision of including the set of controls in the post-matching analysis does not change any of the results presented here. Year FE and Country FE are year and country fixed effects. The last column drops observations that appear only once within a country.

with its unadjusted value of 5.1%. While Model 1 has the advantage to provide easily interpretable coefficients, Model 2 appropriately models the relationship by using a logit link. As expected, the effect remains statistically significant and negative. To provide a further robustness test for the relationship, models 3 and 4 add year and country fixed effects, respectively. The estimated relationship remains negative and statistically significant at a 99% confidence level. Overall, the matching procedure provides further validity to the causal

effect of leaders' Western-based tertiary education on the reduction of their countries' war involvement.

A Further Test of the Mechanism: US versus non-US Western Education

One of the empirical challenges to test the proposed theoretical reasoning is the lack of observable implications that would allow us to directly test the value-driven mechanism. In a recent review paper, Dafoe et al. (2014) argue that leaders often invoke the need to protect national honor, reputation for resolve, respect, and the like, a so-called culture of honor, as a motive for war involvement. Following this line on inquiry, Dafoe and Caughey (2016) exploit the within-country variation in culture between northern and southern United States to estimate whether culture of honor affects war involvement. In particular, the authors argue that white southerner presidents should be more likely to go to war because their cultural background is rooted in the culture of honor, compared to northern presidents. This inter-regional variation within the United States suggests that US cultural values are a combination of peace-prone northerner culture, similar to those found in non-US Western countries, and a war-prone southerner culture. Consequently, the peace-prone emancipative values should be less effective in this particular country due to this mixture of values.[30] If the mechanism of the finding is through change in values, we should then expect that the main relationship is weaker if a leader studied in the United States compared to other non-US Western countries.

To test this proposition, I construct two dummy variables. The first dummy variables takes the value of 1 if the leader has US-based Western education, and 0 otherwise. The second dummy variable takes the value of 1 if the leader has non-US-based Western education, and

[30]In addition, some may argue that the environment of the 1940s, 1950s and 1960s in the United States were not particularly peace-loving given the wide support for the Korean and Vietnam wars. Hence, a finding that the effect is weaker among US-based former students than among non-US-based but Western-educated students would further support the value-driven mechanism.

Table 1.3: The effect of US education and non-US Western education on the initiation of militarized disputes

	Dependent variable: Interstate Dispute Initiation					
	(1)	(2)	(3)	(4)	(5)	(6)
Western US education	−0.60	−0.23	−0.24	−0.17	−0.31	−0.07
	(0.32)	(0.27)	(0.31)	(0.27)	(0.31)	(0.24)
Western non-US education	−1.47***	−1.03***	−0.97***	−0.91***	−0.95***	−0.81***
	(0.28)	(0.24)	(0.25)	(0.24)	(0.25)	(0.23)
Leader country controls	N	N	Y	N	Y	Y
Time-variant country controls	N	N	N	Y	Y	Y
Time-invariant country controls	N	N	N	Y	Y	N
Random/Fixed Effects						
Leader RE	Y	Y	Y	Y	Y	Y
Year RE/FE	N	FE	FE	FE	FE	FE
Country RE/FE	N	RE	RE	RE	RE	FE
Constant	−2.23***	−3.28***	−3.76***	−0.97	−1.07	−62.8
	(0.12)	(0.58)	(0.74)	(0.92)	(1.05)	(40.9)
N	6,209	6,209	6,209	6,209	6,209	5,258
N Countries	147	147	147	147	147	111
N Years	55	55	55	55	55	55
N Leaders	902	902	902	902	902	768
LL	−2.385	−2,240	−2,236	−2,257	−2,213	−2,043
AIC	4,778	4,598	4,616	4,649	4,588	4,460
BIC	4,805	4,995	5,101	5,108	5,134	5,695

Note: *p<0.05; **p<0.01; ***p<0.001. Models are mixed effects logistic regression models with random intercepts by leaders, and random or fixed effects by year and country, depending on the specification. Controls are the same as those included in Table 1.1 GPDpc, ethnic fractionalization and student flow are included in their logarithmic scale since this is the most appropriate functional form in the relationship between these variables and the outcome variable.

0 otherwise. Therefore, those leaders that have non-Western-based education are in the excluded category. Table 1.3 consistently shows that the effect of studying in the US is negative, although not statistically distinguishable from non-Western education. By contrast, the effect of non-US-based Western education is also negative, but it is reliably estimated.

Testing The Endogeneity of Leader Selection

I now return to the issue of endogenous leader transitions that I referred to in the methods section. A potential challenge to the results presented in Table 1.1 has to do with the leader selection process being endogenous to the international security environment. Thus, if a

Table 1.4: Logit Regressions of War Initiation and Western Education (Random Leader Transitions)

	Dependent variable: Interstate Dispute Initiation					
	Deaths-in-Office Sample (All)			*Deaths-in-Office Sample ($\leq$ 5 years since death)*		
	(1)	(2)	(3)	(4)	(5)	(6)
Western education	-0.99^{***}	-0.86^{**}	-1.52^{**}	-1.68^{***}	-1.59^{**}	-1.87^{*}
	(0.24)	(0.42)	(0.68)	(0.48)	(0.74)	(1.00)
Random Effects						
Leader	Y	Y	Y	Y	Y	Y
Year	N	Y	Y	Y	Y	Y
Country	N	N	Y	N	N	Y
N	835	835	835	367	367	367
N Countries	62	62	62	62	62	62
N Leaders	92	92	92	92	92	92
N Years	119	119	119	119	119	119
LL	−414.86	−305.30	−299.45	−169.81	−138.19	−136.68
AIC	833.73	618.59	608.89	343.62	284.38	283.36

Note: *p<0.1; **p<0.05; ***p<0.01. Models 1 and 4 are logistic regression estimates, and Models 2-3 and 5-6 are multilevel logistic regression models with varying intercepts. Constants are omitted from the output.

country's powerful actors believe the country is likely to face a military challenge in the near future, they could decide to change their country leader on the basis of whether the leader was educated in a Western democracy or not. Although it is implausible that leaders are selected during times of turmoil as a consequence of whether their educational background was in the West or not, I show here that the main finding is robust to this possibility by estimating a model that includes only those leaders who entered office in a regular manner immediately after the natural death of their predecessor. Given that there are only a few deaths-in-office transitions, I expand the time range of the sample back to 1875, by analyzing all leader transitions included in Archigos from 1875 to 2001. Even though control variables are not available for such a period of time, confounding factors are partly captured by the inclusion of country and year fixed-effects. In any case, leaving only the fixed effects in place here is a worthwhile trade-off for alleviating the concern of endogenous transitions.

Table 1.4 estimates the main relationship for the deaths-in-office sample without random effects, with year and country random effects, and year, country, and leader random effects (columns 1-3).[31] Regardless of the specification, the results show a strong relationship between Western democratic education and war involvement. The magnitude of the coefficient parallels the general models shown above in Table 1.1. Notwithstanding this, leadership transitions may be random only during some years and, as time goes by, staying in office or not after a period of time can no longer be assumed to be random. To further alleviate this concern, I limit the analysis to the five years after the transition of the leader. Even though the number of observations drastically shrunk, the main association in this deaths-in-office sample restricted to the first 5 years after the leader transition is substantively unaltered, showing a significant negative association between a country leader's Western education and war involvement. Therefore, leaders seem to play an important role in a country's war involvement, and not as a consequence of the factors that let leaders rise to power.

1.4 Conclusion

Following earlier requests for further studies on how the backgrounds of leaders may affect the way nations behave (Horowitz, Stam and Ellis, 2015; Fuhrmann and Horowitz, 2015), this article has explored the impact of prior background of national leaders and a country's likelihood of engaging in interstate disputes against other countries. The first attempt to connect national leaders' education and their country's initiation of interstate disputes found null results (Horowitz, Stam and Ellis, 2015). This paper generally argues that it is not the values, the skills, or the networks that education alone provides to leaders, but that the kind of education is what matters in bringing the values, the skills, or the networks, that

[31] Due to the low number of observations, there is too little variation within years and within countries in leaders' characteristics and war involvement to properly estimate the fixed effects models. Hence, I restrict my empirical analysis here to random-effects multilevel models with year, country, and leader second-level variances.

are associated with particular foreign policies. In particular, this article puts forward that Western education provides with the values, the skills, or the networks, to Western-educated non-Western leaders that influence them toward a more peaceful foreign policy.

The results have shown that leaders who have a university degree from a Western democratic country have a probability of being involved in a militarized interstate dispute that was initiated by the leader's country of 7.3%, compared to the 12.3% probability of an otherwise similarly leader who obtained her degree from a non-Western democratic country. The effect of Western education is significant across a number of different specifications. Mainly, the main effect remains after adjusting for a number of time-variant characteristics of the country – e.g., democracy of political institutions, economic development, number of outbound international students, conflict history, and military capabilities – and year fixed effects or country-specific time trends, as well as after adjusting for time-invariant observable factors – e.g. ethnic fractionalization, colonial legacy, and distance to the West –, and time-invariant unobservable characteristics with country fixed-effects. In addition, the effects are also largely unchanged after matching the observations on observable characteristics, or after changing my definition of the West to include either only Western countries that have been permanently democracies between the period 1946–2001 or only long-lived Western and non-Western democracies, or both.

The study has also made additional efforts to alleviate reasonable concerns about the causal validity of the main findings of the paper by controlling for a number of country characteristics, for pre-treatment characteristics of the leaders, accounting for country and year fixed effects, excluding Western non-democratic countries from the sample, including non-Western democratic countries from the sample, re-coding war involvement to account only for those wars begun by the leader in power, and testing for the possibility of endogeneity related to leader transitions. Regardless of how the empirical models are specified, leader

educational background in the West is associated with the likelihood of a country to become involved in an interstate dispute.

A caveat to the current analysis is that leaders' experiences in the West are not homogeneous. First, contact alone may just be just a necessary condition for socialization, and the quality of contact, an important component for socialization, depends on the visiting students (Merritt, 1972). While it may be the case that students' self-selected social contact is limited to other foreign students with a similar socialization background (Freyburg, 2015), it is also true that socialization in the culture and the social structure of a Western democratic country is more likely if an individual lives in a Western democratic country than if the students stays in her home country. Yet, understanding how the leader-specific experiences in the West shape their shift in preferences remains as a puzzle for further research. Second, this research effort has not been able to compare the experience of international education with other types of experiences that may also shift leaders' preferences such as the involvement in cross-national activities or exposure to foreign media broadcasting (Freyburg, 2015). Finally, another component that this piece has not been able to address is the extent to which the change in foreign policy is due to the Western or the democratic nature of the host countries. With the spread of democratic political institutions around the world, an exploration to national leaders who studied after the third wave of democratization in non-Western democratic countries would allow us to disentangle the Western and the democratic component.

I acknowledge that claiming causality between a leader's educational background and the way a nation behaves from an observational study is a challenging endeavor. However, the analysis and the robustness checks presented here strongly suggest that the link between a leader's educational background in a Western democracy and a country's war involvement is not simply a function of which countries select Western-educated leaders, the temporal evolution of worldwide educational trends and interstate disputes, or the way leaders rise

to power. Consistent with the theory presented here, a Western democratic educational background of leaders shapes the likelihood of leaders to initiate militarized international disputes.

Chapter 2

The Long-Term Effects of War on Civic Engagement: Evidence from the Vietnam War

Abstract

What are the legacies of war exposure on civic engagement? Most research had focused on the short-term effects of domestic conflict on participation in social organizations. I extend this work and argue that exposure to war-related violence transforms people's psychological makeup in a way that increases their civic engagement over long periods of time. This process takes place via, at least, people's participatory values. The Vietnam War offers a unique opportunity to test these propositions and explore the relevance of alternative mechanisms. Specifically, I exploit the distance to the arbitrarily drawn border at the seventeenth parallel as an instrument for conflict intensity. The results show that individuals who lived in a province heavily affected by the conflict during the war tend to be more engaged in social organizations 26 years later. The psychological mechanisms offer the most plausible explanation for the persistent effects of war on civic engagement.

The short-term consequences of war are dreadful: the destruction of physical infrastructure, the weakening of economic and political institutions, and the obvious losses in human lives, among others. While the dramatic short-term consequences of war are self-evident, there is no consensus about the long-term consequences of war on societal and developmental outcomes. The conventional wisdom claims that war unavoidably and drastically alters development and social cohesion. Early developmental researchers describe wars as "development in reverse": their legacy is the persistence of under-development through the weakening of local and national political institutions, the destruction of the social fabric, and division of populations by removing the foundation of norms, values, and interpersonal and communal group trust that facilitate inter-personal cooperation (Colletta and Cullen, 2000; Collier et al., 2003). Consistent with this perspective, some micro-level researchers have found that armed conflicts negatively affect tangible factors such as individuals' investment, income, and consumption (Blattman and Annan, 2010; Deininger, 2003), as well as less tangible elements such as psychological well-being and social trust (Cassar, Grosjean and Whitt, 2013; Kijewski and Freitag, 2018).

Notwithstanding this, conflict could also spur institutional innovations or alter preferences in a way that improves societal outcomes (Cramer, 2006; Leonard, 2004; Morris, 2014; Tilly and Ardant, 1975). A small body of micro-level evidence is now emerging that is consistent with this latter proposition. Those citizens who have been more heavily affected by a civil war seem to engage in greater collective action in the aftermath of the civil conflict in Burundi (Voors et al., 2012), Nepal (Gilligan, Pasquale and Samii, 2014), Sierra Leone (Bellows and Miguel, 2006, 2009), and Uganda (De Luca and Verpoorten, 2015).[32]

However, such studies are limited in their ability to fully address the relationship between violence exposure and civic engagement. First, much of this literature remains theoretically

[32]Other research has focused on the perpetrators of violence. Cassar, Grosjean and Whitt (2013) show that combatants are more prosocial than non-combatants 13 years after the war and Blattman (2009) provides evidence that abducted child soldiers in Northern Uganda are more politically engaged once they return.

and empirically agnostic about the pathways leading to behavioral changes. Second, outcomes are usually measured in the aftermath of war—usually within the first decade after the end of the violence—in the midst of lingering violence or reconstruction efforts, which could bias the results in favor of more prosocial effects. Third, the non-random geographical distribution of conflict violence hampers our ability to establish a causal relationship from conflict exposure to preferences. Fourth, most evidence has thus far been limited to civil wars—in which perpetrators are domestic actors—where increased engagement may be associated with heightened inter-group tensions (Bauer et al., 2016; Mironova and Whitt, 2016). Fifth, differences in outcomes across communities in post-conflict settings may be due to the effect of war but also due to non-response bias (e.g., death, post-conflict) (Gilligan, Pasquale and Samii, 2014). Finally, most projects rely upon retrospective self-reported conflict victimization data, which may be subject to recall and non-response bias (Brück et al., 2016; Child and Nikolova, 2018).

This paper offers both theoretical and empirical contributions to the current literature that overcome these limitations in the same project. Theoretically, I suggest a mechanism that may partly account for the long-lasting legacy of political violence on individuals' participation in social organizations. I argue that exposure to civil war-related violence leads to a transformation in people's psychological makeup, which affects their long-term civic engagement. This process takes place via, at least, a process whereby individuals who are more exposed to the conflict change their preferences to give more importance to abstract values such as freedom of speech, people's efficacy in how things are managed in their proximate community, the society, and the government.

Empirically, I use the case of the Vietnam War to overcome the empirical challenges. I identify the long-lasting effects of war exposure by exploiting the arbitrarily drawn border along the 17th parallel between North and South Vietnam in 1954. The research design is complemented with two unique data sources on the Vietnamese conflict: (1) the geographic

distribution of the U.S. military campaigns drawn from a dataset containing province-level bombing intensity; and, (2) survey data collected in 2001 by the World Value Survey, which include respondents' province of birth, province of residence just before the end of the war in 1975, and place of residence for the period of 1990-2001. The main finding of this paper is that those respondents who lived during the Vietnam War in a province that was heavily affected by the conflict are more likely to be engaged in civic organizations in 2001, 26 years after the end of the conflict. In addition, I provide consistent evidence that a plausible mechanism for this effect is an increase in people's participatory values.

These findings are robust to different measurement decisions in the outcome variables, removing individuals' self-selection during the war, accounting for selection bias in the sample due to death and migration, and only using the sub-sample of respondents from North Vietnam, those who did not experience a regime change. In addition to the main findings, the richness of the data allows me to investigate the psychological mechanisms of these associations. While I find suggestive evidence for the role of an increase in participatory values; I can rule out alternative mechanisms such as changes in the sociological composition of communities due to unequal survival or migration, postwar state investments, unequal cross-regional development, and overall changes in the post-conflict context. Finally, I conclude that psychological pathways offer the most plausible explanations for the long-lasting effects of war-related violence on civic engagement.

2.1 The Behavioral and Attitudinal Legacies of War

Early scholarship on the effects of war on people's psychology portrays a gloomy picture. Most studies report that survivors of trauma, even many years after the events, often suffer from a wide range of physical and mental health problems, such as lower physical well-being

and signs of post-traumatic stress disorder, including anxiety and depression (Bonanno et al., 2006). These results, in turn, are generally associated with hopelessness about the future, feelings of threat, and social withdrawal (Galovski and Lyons, 2004), all of which would lead to lower levels of civic engagement.

While PTSD may affect victims of warfare violence after a traumatic exposure to conflict, an increasing number of scholars posit that not only do most citizens recover quickly (Murthy and Lakshminarayana, 2006) but also that exposure to the conflict may transform people's psychological make-up (Boehm-Tabib, 2016; Knobler et al., 2016; Tedeschi and Calhoun, 2004; Tsai et al., 2015). These psychological changes may facilitate short- and long-term civic engagement by bolstering individuals' participatory values.

2.1.1 The Legacies of War on Participatory Values

In the realm of the psychological theories of war exposure, a widely shared theoretical framework, the post-traumatic growth theory (PTG), could account for a long-term link between war exposure and civic engagement (Blattman, 2009; Tedeschi and Calhoun, 2004). The PTG refers to "positive psychological change experienced as a result of the struggle with highly challenging life circumstances" (Tedeschi and Calhoun, 2004, 1). The process of traumatic growth begins with the idea that everyone holds a set of beliefs and assumptions that they depend on to make sense of their world. These beliefs guide analytical thinking, understanding of causes and effects, and direct people's courses of actions. A traumatic event is then defined as a major challenge to this assumptive world. After the traumatic event, individuals struggle with the new reality by rebuilding a new set of assumptive world dictums that incorporate the trauma and the potential that similar events could happen in the future. The result of the entire process is that individuals respond to trauma by reflecting on and reassessing their lives, particularly by changing their social preferences. These changes lead

to an augmented perception of social connectedness, self-efficacy, and personal strength to exercise control over events that affect their lives, as well as an increase in the importance given to life, to other people, and to intimate relationships (Boehm-Tabib, 2016; Calhoun and Tedeschi, 1999; Knobler et al., 2016; Linley and Joseph, 2004).

Post-traumatic growth involves a process of positive psychological change that sets in motion a complex process of re-structuring preferences that are strongly connected to major well-known triggers of civic participation. First, an augmented perception of an individual's self-efficacy is related to the concept of political efficacy; that is, citizens' beliefs about "one's own competence to understand and participate effectively" (Niemi, Craig and Mattei, 1991, 1407). A vast literature in political behavior supports the thesis that an individual's beliefs that one can make a difference in the social and political realm are strongly associated with stronger preferences for participating in public affairs, hence participatory values, as well as with actual levels of civic engagement (e.g., Abramson, 1983; Almond and Verba, 1963).

Similarly, PTG may lead to an increase in people's "expressive preferences," that is, the value that citizens give to the act of participation in public affairs (Blattman, 2009). Participation may be viewed as an intrinsically valuable act regardless of the decisiveness of that behavior because it is a channel to reaffirm beliefs, values, and identities, (e.g., Schuessler, 2000). Wood's (2003) ethnographic work in El Salvador shows that the experience of war-related violence increased the peasants' value of associational life and collective action.

Overall, exposure to war-related violence is likely to lead to positive psychological change by re-shaping their structure of preferences on a number of dimensions that are relevant for their future civic engagement. These changes are reflected in an increase in their sense of efficacy, their intrinsic pleasure of political expression, and pro-social preferences, which, altogether, should promote beliefs about the value of participating in public life.

Hypothesis of the Long-Term Persistence of War Effects

Though most of the theoretical mechanisms directly speak to the immediate effects of exposure to war on public engagement, there are reasons to believe that they may have persistent effects. In the long-run, political events that exert psychological transformations are likely to crystallize in a new psychological make-up that is sustained over time (Henry and Sears, 2009; Lelorain, Bonnaud-Antignac and Florin, 2010; Sears, 2002). Therefore, the new set of preferences that results from the PTG process should be sustained in the long-term through this crystallization mechanism.

Second, social preferences are difficult to reverse, socially reinforced, and transmitted across generations. In this regard, Nunn and Wantchekon (2011) find that individuals whose ancestors were subject to heavy raids during the slave trade four centuries ago are less trusting of their relatives, neighbors, out-group members, and political institutions today.

More generally, the legacies of political violence have been found to influence contemporaneous authoritarian values, preferences toward those who are related to the perpetrators and social identities directly, through personal experiences, and indirectly, through their transmission across generations (Hong and Kang, 2015; Lupu and Peisakhin, 2017; Rozenas, Schutte and Zhukov, 2017). Therefore, the effects of war-related violence on civic engagement are also likely to persist even decades after the end of the conflict through inter-general transmission of values and attitudes. This leads to the following main hypothesis:

Hypothesis 1: *Exposure to war violence* enhances *individuals' civic engagement in the long-term.*

In addition, the psychological mechanisms suggest that those respondents who were more exposed to the war may generally place greater value in the act of civic participation itself.

Hence, a key empirical implication of this mechanism would be that these individuals would also report a high priority for achieving greater levels of freedom of speech, expanding the space for civic engagement, and greater value in the role of people's influence in public affairs, which is stated in Hypothesis 2:

Hypothesis 2: *Exposure to war violence* enhances *individuals' participatory values in the long-term.*

2.2 Threats to the Identification of a Causal Effect

A correlation between war exposure and civic engagement may not reflect a causal effect because of potential reverse causation or omitted variable bias. This applies when analyzing both short and long-term effects of exposure to war. The most obvious issue is that conflict intensity, and thus exposure to war-related violence, is not random. For instance, areas with well-organized groups, with a more active citizenry, or with more individuals holding pro-freedom values may either be more likely to be targeted for violence by the attackers or more likely to defend themselves from a violent attack.

This is arguably the major issue in the literature establishing the short-term effects of war on people's participation. Some scholars have attempted to mitigate its impact by following five different strategies: (1) adding fixed effects at an aggregate unit to maintain constant village specific characteristics if there is within-unit variation (e.g., Bellows and Miguel, 2006, 2009; De Juan and Pierskalla, 2016; De Luca and Verpoorten, 2015); (2) controlling for pre-war communities' characteristics through regression or matching approaches (De Juan and Pierskalla, 2016; Gilligan, Pasquale and Samii, 2014); (3) sub-sampling on most affected populations (Bellows and Miguel, 2006, 2009); (4) providing a qualitative account about why violence onset was random (Gilligan, Pasquale and Samii, 2014); and, (5) instrumenting

for violence with individuals' geographic location (De Juan and Pierskalla, 2016). While these strategies alleviate some concerns of endogeneity, a causal effect cannot be disentangled without a source of exogenous variation in the geographic distribution of war conflict.

A second major concern for the identification of a causal effect is attrition. This has to do with the confounding role of non-random migration or forced displacement of people who move during or after the war, and then do not go back home. Thus, selection bias becomes an issue if those who tend to be more active in social organizations are more or less likely to change their residence due to the conflict. To deal with this, researchers need to separate internal effects—i.e., those effects that work through the individual such as psychological shifts, changes in beliefs, values, norms—from the external effects—i.e. those that work through the context such as the economic, political, institutional, and social structures that may be different between areas affected by the conflict and areas that were not.

The Vietnamese case provides an excellent natural experiment to overcome some of the inferential concerns that are present in other studies and helps elucidate the internal versus external channels of the effects of war. The critical starting point is to fully comprehend what drives the conflict intensity patterns that we observe across provinces—the units of analysis of the Vietnamese conflict data—to identify some discontinuity that produces exogenous variation in bombing that could be exploited for the purposes of causal inference. In addition, the richness of the Vietnamese survey data allow the decomposition of internal and external effects of bombing.

2.3 The Vietnam War and the Border as a Natural Experiment

The Vietnam War (1954–1975) was a conflict officially fought between North Vietnam and the government of South Vietnam. Its origins can be traced back to the end of World War II (WWII). In 1945, the Viet Minh—a Vietnamese opposition movement to the French rule led by Ho Chi Minh—took power in Vietnam in the August Revolution, amidst the vacuum of power left by the defeat of Imperial Japan. However, the allied victors of WWII agreed that Vietnam belonged to the French. The British helped to re-build French control in the area, which allowed them to take over South Vietnam. In addition, the French landed in Hanoi and ousted the Viet Minh. Soon after the British departed in 1946, the Viet Minh initiated a rural counter-insurgency guerrilla war against the French government with the support of the Chinese communists. After the guerrilla took control of the northern border of Vietnam in 1949, the war became the First Indochina War. Though the conflict engulfed the entire Indochina area, the Red River Valley in Tonkin, northern Vietnam, was the geographic location of the most intense fighting between the French army and Viet Minh forces (Schulzinger, 1997, 44–68).

Following the defeat of the French army (May 7, 1954), the Soviet Union, the United States, France, the United Kingdom, and China participated in the Geneva Conference to restore peace in Vietnam (May 8, 1954). At first, none of the parties thought that a partition was possible in Vietnam, and France and the United States were opposed to it.[33] Yet, the

[33]On the one hand, Bao Dai, the chief State of Vietnam, rejected "partition, direct or indirect, definitive or provisional, in fact or in law, of the national territory" (Logevall, 2012, 562). Eisenhower thought similarly. In fact, the CIA director, John Foster Dulles, is believed to have said to the cabinet and privately to his brother that he thought that it was not possible "to draw a line, given both the present balance of forces and the geography of Indochina" (562). Alternatively, France suggested the physical separation of combatants but in the form of enclaves controlled by each side, the so-called leopard-skin approach, French forces would retain control over the Red River Delta, including the major urban areas of Hanoi and Haiphong (562–563). These statements reflect the positions of the parties as on May 12, 1954.

turnover of the French government in June, as well as the military defeats on the ground, led to a moderation of the French position; the French would now accept a partition.[34] In June, negotiations focused on the exact location of the partition, which was uncertain at that time.[35] Finally, the Geneve Accord, which was struck on July 21, 1954, set out a "provisional military demarcation line" running through the seventeenth parallel.

Some characteristics of the entire process are fundamental for identification purposes. First, the seventeenth parallel had not been used before for any other circumstance of political relevance. Second, the parallel did not coincide with areas of conflict throughout the First Indochina War, when most of the conflict occurred around the River Red Delta and the Chinese border, around the twenty-first parallel and north. Third, it was unclear before the beginning of the negotiations in 1954, whether Vietnam would be partitioned, and, even more importantly, the specific parallel that would be used for partition was uncertain. Fourth, the parallel did not reflect the relative forces of the parties on the ground.[36] All historical evidence with which I am familiar suggests that the border was arbitrarily drawn and the pre-existing geographic distribution of Vietnamese people's political attitudes or behavior on the ground played no role in the location of the border.[37]

Though quite arbitrary, the border had an enormous impact on the subsequent Second Indochina War, or Vietnam War (1954–1975). The first stage of the war was characterized by a low-intensity insurgency war in South Vietnam, and the military intervention of the United

[34]The United States withdrew from major participation of the convention in early-mid June.

[35]On the one hand, the Democratic Republic of Vietnam called for the partition line to be at the thirteenth parallel (595–596). On the other hand, understanding that Ho Chi Minh would only accept an agreement if their territory would include Hanoi, the French suggested a division at the eighteenth parallel, which runs south of Hanoi.

[36]A member of the French delegation remarked "one is entitled to think that the division of the country at the thirteenth parallel would have more accurately reflected the true state of affairs than the partition at the seventeenth parallel which we achieved" (608).

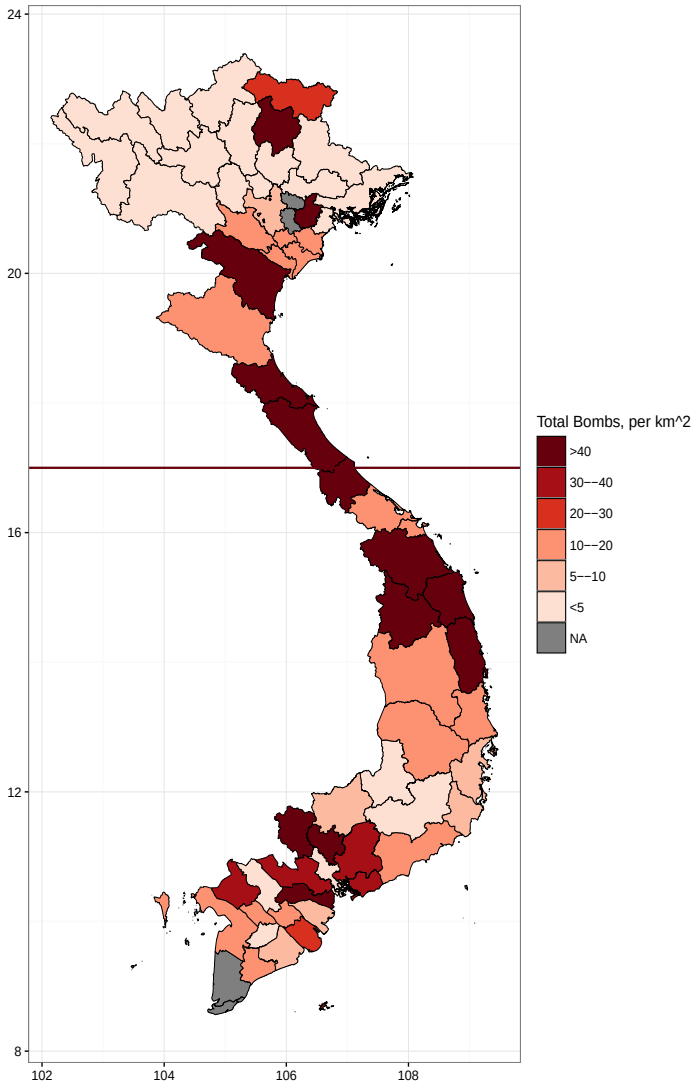
[37]On the contrary, the First Indochina War and the numerous communists' hubs in the northern border with China suggests that, if anything, the most socially and politically engaged people would be located in the urban and northern areas. Thus, they are geographically far from the middle areas of the country.

States escalated in 1964.[38] Between 1964 and 1973, the conflict was characterized by stark asymmetries of firepower, and a massive U.S. air bombing campaign. Bombing in Vietnam was indiscriminate; that is, targets were not chosen on the basis of individual characteristics, but bombings targeted areas where civilians lived and, thus, collectives of civilians rather than specific individuals (Kocher, Pepinsky and Kalyvas, 2011). In a context of indiscriminate bombing, aggregate bombing data of an area captures the average exposure to the war of a region's inhabitants. To illustrate the geographic variation of the impact of the Vietnam War across throughout the Vietnamese territory, the map in Figure 2.1 shows the distribution of bombings across provinces.[39] In addition to some areas around Hanoi, the coastline of North Vietnam, and the area around Ho Chi Minh City that are closest to the Cambodian border—the end of the Ho Chi Minh trail that connected North and South Vietnam through Laos and Cambodia—the most intensely bombed regions are close to the 17th parallel of the North-South border. Given the strong relationship between bombing intensity and distance to the 17th parallel and the arbitrary manner in which the border was setup, I follow Malesky and Taussig (2009) and Miguel and Roland (2011) by instrumenting the intensity of the bombing with distance to the border to estimate the causal effect of bombing on post-conflict outcomes.

[38] After the Gulf of Tonkin Resolution, on 7 August 1964, which allowed the U.S. president to launch a full-scale war if it was considered necessary by the president.

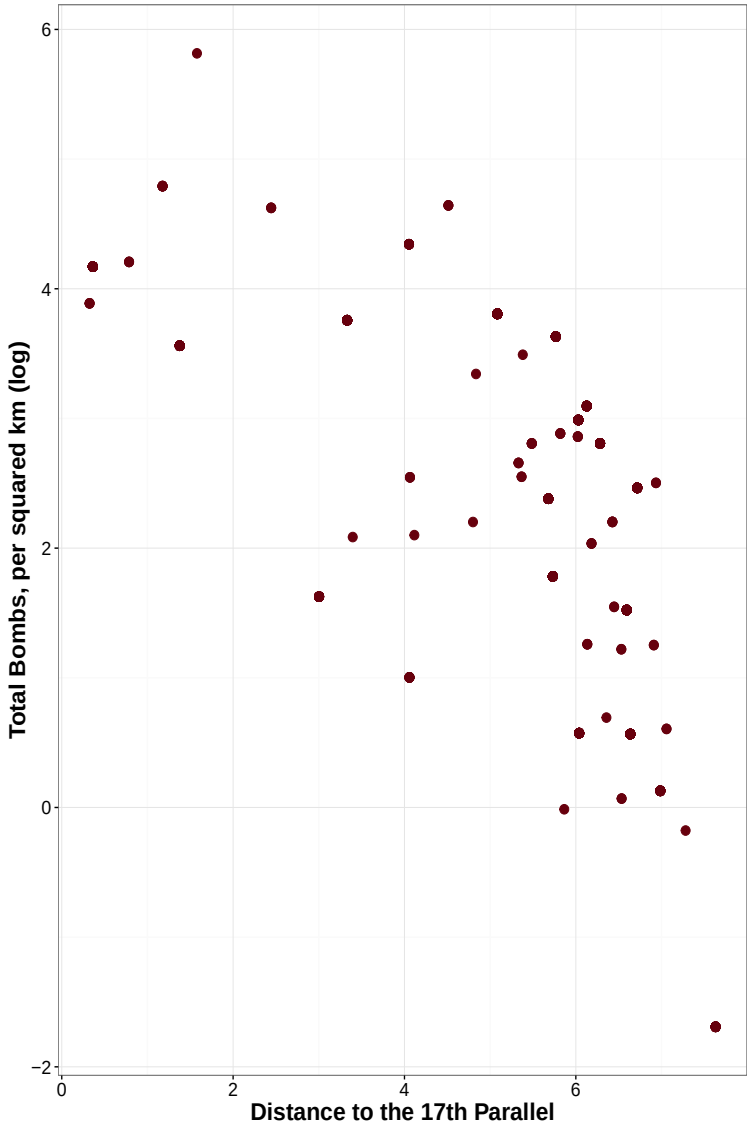
[39] See below for a further discussion on the size of provinces. More details on the bombing data can be found in the methodological section below.

Figure 2.1: Geographic Distribution of the Bombing Campaign in Vietnam and Distance to the 17th Parallel



Note: The map includes only those provinces with available bombing data.

Figure 2.2: Bombing Intensity and Distance to the 17th Parallel



Note: The scatterplot includes only those provinces with at least one respondent from the WVS survey used in the analysis.

2.4 Data and Measures

I rely on the first public opinion survey ever conducted in Vietnam, the World Value Survey 2001 (WVS).[40] To the core questionnaire of the international WVS project, the Vietnamese survey adds a set of country-specific questions on socio-economic development that are relevant for the object of this study, including an extensive battery of items on civic engagement.

In addition, the survey also includes specific questions on the respondents' province of birth, place of residence before 1975 (the year of the end of the Vietnam war), and in the period 1990–2001 (year of the survey).[41] These questions allow me to match each respondent to the province-level conflict data.

2.4.1 Key Measurements

I construct my main dependent variable: *Civic engagement*, and a plausible mechanisms: *Participatory values*. Finally, I construct an independent variable: *War exposure*, and an instrumental variable: *Distance to the 17th parallel*.

Civic Engagement

In order to measure civic engagement, I use respondents' self-reported participation in social groups. Specifically, respondents were asked to indicate whether they were doing unpaid voluntary work in 14 types of voluntary associations or activities offered in the questionnaire: (1) social welfare services for the elderly, handicapped or deprived people; (2)

[40]The survey in the field was conducted by the Institute of Human Studies in Vietnam. While some academics criticize the WVS for its lack of comparability across countries, these considerations do not affect analysis within a country sample (Kurzman, 2014).

[41]This set of questions was included by the researchers to meet the Institute's own research program, but not available in the public WVS data files. I thank J. Diez for access to the original data files.

religious organizations; (3) education, arts, music or cultural activities; (4) labor unions;[42] (5) political groups or organizations;[43] (6) local community action on issues like poverty, employment, housing, racial equality; (7) third world development or human rights; (8) conservation, environment, animal rights groups; (9) professional associations; 10) youth work (e.g. scouts, guides, youth clubs etc.); (11) sports or recreation; (12) Women's groups; (13) peace movement; and, (14) voluntary organizations concerned with health.[44]

The online Appendix B.1 reports the question wordings and descriptive statistics for these items (panel B). While most respondents do not participate in any social organization, about one fourth of the respondents report engagement in community action organizations, labor unions, Women's groups, or organizations that provide welfare services. As suggested by earlier Vietnamese scholars (Nørlund, 2007; Taylor et al., 2012; Wischermann, 2013), these descriptive statistics show that Vietnam has a generally strong broad-based civic society with relatively high levels of civic participation.[45]

To estimate scores for each respondents on separate scales of participation on civic organizations, I apply a binary factor analysis (FA) based on a matrix of tetrachoric correlation coefficients. Results from the binary FA suggest a one-factor structure for the participation items.[46] Due to the skewness of the distribution of the index—most people report not being a

[42]Though labor unions in Vietnam are formally controlled by the Communist Party; membership is voluntary and, in practice, workers are autonomous with respect to the national party. This explains the prevalence of wildcat strikes among Vietnamese workers (Chi and Broek, 2013).

[43]While political groups or organizations could be considered to belong to national formal institutions, the party structure in Vietnam is highly decentralized. This means that local party organization or groups are, for most of their activities, highly independent with respect to the national party leadership. Because citizens are much more likely to devote their time to a local party organization rather than to the national organization, we should consider it as not belonging to national formal organizations. Yet, excluding this category from the list does not alter any of the results or conclusions of this paper.

[44]A residual category of "other groups" is also included in the survey, although not used in the analysis.

[45]Specifically, associational life in Vietnam surpasses that of other authoritarian countries such as Singapore or China (see Norlund, 2007: 78).

[46]The Cronbach's alpha of the participation items is 0.79. Table B.1 (panel B) in the online appendix reports the factor loadings of each item that are used to compute respondents' score in the index of participation.

member or an active participant of any organization or type of activity—I take the log of the first factor from the binary factor analysis as the dependent variable of civic engagement.[47]

Participatory Values

To measure participatory values, I use a sub-index of the Post-Materialist Index, called "pro-voice values." This sub-index intends to measure respondents' priorities for freedom of speech and people's say in national and local affairs (Welzel, 2013*b*, 66-69). Respondents were asked to choose the statements that best reflect their life priorities among eight items (two four-item blocks). Of the eight items, three measure an emphasis on people's participatory values: (1) "Seeing that people have more say about how things are done at their jobs and in their communities," included in Block 1; (2) "Giving people more say in important government decisions," and, (3) "Protecting freedom of speech," both included in Block 2.[48] Responses from each block are recoded to 0 when the item has not been chosen as important, 0.5 when it has been chosen as second most important and 1 when it has been chosen as most important.[49]

Bombing Intensity

The bombing data used to construct an individual-level measure of war exposure were initially assembled by Miguel and Roland (2011), and derived from three different sources: (1) records of naval gunfire support in North and South Vietnam (March 1966—January 1973); (2) the Combat Air Activities File (CACTA), which details daily air combat operations flown by the US Navy, Marine Corps, and Pacific Air Forces, by fixed-wing aircraft from October 1965

[47] Results remain substantively unaltered if I proceed with the natural scale.

[48] Because their inter-item correlation is moderately low, $r = 0.35$, I ensure that the validity of the Index does not jeopardize the results by reporting the results for the two blocks of the Voice Index separately. For further information on the variable, coding procedures, and a full Table with the three four-item batteries that were used in the original WVS questionnaire, see the online Appendix B.4.2.

[49] The coding procedure follows (Welzel, 2013*b*, 66-69) coding instructions and is imposed by the way in which the WVS asks these questions.

through December 1970;[50] and, (3) daily records of allied air combat operations by fixed-wing and helicopters flown by the U.S. Army, (South) Vietnamese Air Force, Royal Lao Air Force, and Khmer (Cambodian) Air Force between 1970 and 1975.[51]

The bombing data includes the number of general purpose bombs, cluster bombs, missiles, rockets, cannon artillery, incendiary bombs, white phosphorus and ammunition (in thousands) dropped in each province per square kilometer.[52] The total number of ordnance per square kilometer dropped in a province constitutes the measure of the province-level war intensity. In the online Appendix, Table B.3 reports the correlation matrix between the summed index and the different types of bombs available in the dataset, and Table B.2 reports the descriptive statistics of each of the ordnance category per square kilometer. The index of conflict intensity is highly correlated with all types of ordnance, especially general purpose bombs. Hence, the total number of ordnance provides a good measure of bombing intensity in a province.

To construct an individual-level measure of exposure, I match the bombing data to respondents' information about their province of residence before 1975, which is included in the WVS 2001 survey in Vietnam.[53] Unlike earlier work that relies on self-reported measures of victimization, which are prone to recall bias and non-response bias (Brück et al., 2016; Rosenfeld, Imai and Shapiro, 2015), I use an objective measure of exposure based on their residence during the war. Another important consideration for evaluating the empirical results is the number and size of the units. Vietnam is divided into relatively small units. The entire country has a surface area of $128,455 mi^2$, which is similar to that of American states

[50] A bombing sortie usually included multiple aircraft and weapons. The CACTA was assembled from post-flight pilot debriefs conducted on the day of each sortie.

[51] The datasets are hosted by the U.S. National Archives and access is unrestricted.

[52] Ordnance is measured in units.

[53] The sample in the models include all respondents that provide a valid response to a place of residence before 1975. Though most respondents younger than 26 years old have a missing value in this question, 62 (out of 184) respondents who are too young to have experienced the war themselves have valid responses. The main analyses are implemented using all of those whose response is valid regardless of their age. All results are unaltered if I exclude these respondents from the sample.

such as New Mexico. Each province has an average area that is smaller than the average area of counties in New Mexico.[54] The small size of provinces reduces the potential measurement error around the independent variable.[55] In any event, the use of an instrumental variable will also help to alleviate issues related to measurement error. The sample geographically encompasses the Vietnamese population and the disparity in conflict intensity. Of the 63 administrative units existing in today's Vietnam, there are 49 provinces that both can be matched to the codes in the bombing records and have at least one respondent in the WVS dataset that reports having lived in the province during the war.[56]

Instrumental Variable: Distance to the 17th Parallel

The main empirical concern to estimate a consistent effect in the hypothesized relationship is that conflict intensity and civic engagement may be endogenous. First, bombing can be targeted to those areas with greater mobilization, or more actively involved people. Second, both a province's conflict intensity before 1975 and today's civic engagement may result from an unobserved omitted variable. Third, the number of bombing units dropped in each province is an imperfect historical measure of conflict intensity. Though it is highly correlated with true conflict intensity, there are a number of aspects that it does not capture, such as the violence induced by the North Vietnamese army, additional damage from ground operations, and the fact that the measure is the total number of bombs dropped rather than the actual damage inflicted in a province. Therefore, the measure of conflict intensity is prone to measurement error. Due to the existence of these problems, the OLS estimate may be biased and inconsistent.

[54]The average surface area of Vietnamese provinces is $2,039 mi^2$, which is smaller than the average surface of counties in New Mexico.

[55]Though other finer-grained datasets of conflict in Vietnam are currently available (Dell and Querubin, 2018; Kocher, Pepinsky and Kalyvas, 2011; Miguel and Roland, 2011), respondents' location in the survey can only be known at the province level. Therefore, cumulative bombing in each province is the most fine-grained value that can be attributed to respondents.

[56]See the online Appendix B.5 for the number of respondents by province.

The exogenous variation provided by the distance to the arbitrary North-South border may serve as an instrument for conflict intensity (Kocher, Pepinsky and Kalyvas, 2011; Malesky and Taussig, 2009; Miguel and Roland, 2011).[57] The validity of the instrument is based on the fact that the location of the border drawn in the 1954 Geneva Accords was exogenous to the characteristics of the people living in those areas. However, distance to the parallel can itself be endogenous to some geographical or cultural conditions. Hence, distance to the border is an exogenous instrument of bombing intensity conditional on average precipitation, latitude, pre-war population density of the province, and whether the province is located in South or North Vietnam. These controls adjust for geographic and cultural disparities within Vietnam that are correlated with distance to the parallel.

2.4.2 Control Variables

The models also include three control variables at the level of the individual: age, gender, and education.[58] The theoretical rationale is that conflict exposure influences civic engagement through individuals' psychological processes, which may be reflected in some individual-level variables. Yet, if these intervening variables that reflect such attitudinal processes matter, and they are included as part of the model specifications, they may bias the causal estimate of the treatment. Hence, the model includes demographic controls that are immune to the influence of province-level exposure to bombings—such as age and gender—but it does not control for factors that may mediate or be a product of bombing exposure—such as political orientations, current town size, and the like.[59]

[57]Province latitude is the average of the district latitudes weighted by district land area and district latitude is the district centroid.

[58]See Table B.1, Panel A, in the online Appendix for their descriptive statistics.

[59]The control for educational attainment may be post-treatment for some respondents—those whose education occurred after the end of the war. Though the main models include it, none of the results depend on its inclusion.

2.5 Results

Table 2.1 reports OLS and IV estimates of the impact of bombing on the outcome. The OLS estimates reported in columns 1 and 2 suggest a strong positive and significant effect of the number of total bombs per square kilometer in a respondents' province of residence before 1975 on the level of engagement in social organizations, measured as active unpaid work for an organization.[60]

Because there are reasons to believe that the OLS estimates may be biased and inconsistent, models 3 and 4 instrument bombing intensity with the absolute distance of the province to parallel 17. The results from the first stage regression are reported at the bottom of the table both with and without the second-stage controls (controls are included but omitted from the output). The coefficient of the absolute distance to the parallel is significant and negative, which confirms that provinces located closer to the parallel were more heavily bombed than provinces that were located farther away. The bottom panel in Table 2.1 shows that the instrument is significant at conventional levels, and correlated with the endogenous regressor with an F-statistic that far exceeds the suggested threshold of 10.

Consistent with the OLS estimates, the effects of the instrumented bombings are also positively associated with civic engagement in the model without controls (model 3), as well as in the model with controls (model 4). The coefficients are substantially larger compared to the OLS estimates. The increase in magnitude suggests that the downward bias due to measurement error dominates the potential threat from upward bias due to omitted variable bias or reverse causation. Taken together, these empirical findings suggest support for hypothesis 1.[61]

[60] To correct for the non-independent of observations from the same provinces, all models reported in this paper include heteroskedasticity-robust standard errors clustered at the level of the province.

[61] When looking at the effects of war exposure on participation in each social organization, the estimates are significant on 11 of the 14 types of organizations. The three types of organizations that are not associated with exposure to bombing are: professional, young, and Women's organizations. This is reasonable given that

Table 2.1: The Impact of U.S. Bombings on Civic Engagement

	Civic Engagement Index (log)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.08^{***}	0.12^{***}	0.12^{***}	0.23^{***}
(Province, residence pre-1975)	(0.01)	(0.02)	(0.03)	(0.04)
Control Variables				
Education		0.03^{**}		0.02^{*}
		(0.01)		(0.01)
Gender		-0.06^{*}		-0.07^{*}
		(0.04)		(0.04)
Age		-0.001		-0.002
		(0.001)		(0.001)
South		-0.46^{***}		-0.28^{*}
		(0.14)		(0.16)
Latitude ('0)		-0.28^{**}		0.01
		(0.13)		(0.16)
Population density (1960-61) ('000)		-0.05^{**}		-0.11^{***}
		(0.02)		(0.03)
Average precipitation ('00)		0.01		-0.23^{**}
		(0.09)		(0.11)
Intercept	-0.38^{***}	0.25	-0.12^{***}	-0.17
	(0.03)	(0.28)	(0.03)	(0.33)
			First Stage	
Distance to the 17th Parallel			-0.42^{***}	-0.55^{***}
F Statistic (instrument)			483^{***}	354^{***}
Observations	875	862	875	862
Provinces	48	49	48	49
R^2	0.04	0.09		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the dependent variables to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

Figure 2.3, Panel A, illustrates the magnitude of the effect of conflict intensity on civic engagement, after keeping all continuous control variables at their mean value, and categorical control variables at their median value. Because both the dependent and the independent variables are measured in a natural logarithmic scale, we can interpret the coefficients in terms of elasticities. Thus, the estimates suggest that a 10% in the total number of ordnance per square kilometer in a province increases the expected value in the index of civic engagement by 2.3%.[62] In terms of standard deviations, an increase in one standard deviation in the log of the total number of ordnance, i.e., 1.33 logs, is expected to lead to an increase in the log of civic engagement of 0.31, so about 0.56 standard deviations of the outcome.

Panel B in Figure 2.3 transforms the predictor and the predicted values to their natural scale. Because the level of bombing takes very high values in some provinces, the overplotted histogram in Panel B only shows observations with a number of ordnance per square kilometer lower than 30 (65% of the observations).[63] From this, we can see that increasing the number of ordnance dropped in the respondents' province of residence in 1975 leads to substantial increases in the expected values of civic engagement, especially among those respondents who lived in provinces with moderate-low levels of bombing, which constitute most of the sample of respondents. For instance, an increase from 0 to 5,000 ordnance leads to an increase in the index of civic engagement from no participation at all, its lowest value, -0.64, to actively participating in one or two organizations, -0.45.[64] A further increase in the level of bombing to 25,000, about the mean of the distribution, leads to a further increase in the expected value of civic

they are somehow segmented organizations for which you need to work in an organized guild, and be relatively young to belong to the first two. Though everyone can formally participate in a Women's organization, gender—rather than social preferences—is the key predictor for participating in them. In any event, the lack of association with some organization also minimizes the concern of social desirability bias that may exist in responding to these questions.

[62]This value is computed as follows: $e^{0.24*ln(1.1)} = 1.023$, so 2.3%.

[63]See Figure B.3 in the online Appendix for an expanded figure with the full distribution of the number of ordnance in the province.

[64]See Figure B.1 for a mapping of the values in the index of civic engagement to the number of organizations in which the respondent is active.

engagement to -0.25, which is equivalent to actively engaging in 2 or 3 organizations. Finally, respondents who lived in the most heavily bombed provinces, above 100,000, are expected to have a value of civic engagement above 0.20, which is equivalent to be actively engaged in about 4, 5, or 6 organizations.[65] These increases in the expected values of civic engagement are substantial, especially when we consider that the effect took place between two and four decades ago.

2.5.1 Robustness Checks and Sensitivity Analysis

The original survey contained two types of participation in civic organizations: membership and volunteering. The main findings are reported based on the latter, although the results are insensitive to this decision. Table 2.2 reports the results from columns 1 through 4 in Table 2.1 but measured as formal membership in a social organization. The estimates are nearly identical.

Second, one could argue that the survey question regarding the place of residence allows for self-selection into the province of residence. Indeed, while some people may have answered their place of residence during the Vietnam War, others may have provided their post-war place of residence or may have moved during the war and, thus, the self-selection into a relocation province may bias the results.[66] Table 2.2 re-estimates the models using the respondents' place of birth, which is arguably outside the control of the respondents. The main estimates are substantively unaltered.

Another potential concern is that the relationship might be entirely driven by people's behavior in extremely bombed provinces. While the logarithmic scale alleviates the concern

[65] See Panel A in Figure B.3 to see the predicted values for those provinces at the highest value of bombing.

[66] Yet, the issue is less problematic than one could imagine given the size of the small size of domestic migratory movements among the respondents in the survey: a) only 11% of the respondents report a different place of residence before 1975 compared to their place of residence between 1990 and 2001; and, b) only 12% of the respondents report a province of birth that is different from their pre-1975 province of residence. Thus, if self-selection is an issue, its impact is limited.

Figure 2.3: Predicted Values of Civic Engagement at Different Levels of Bombing Intensity

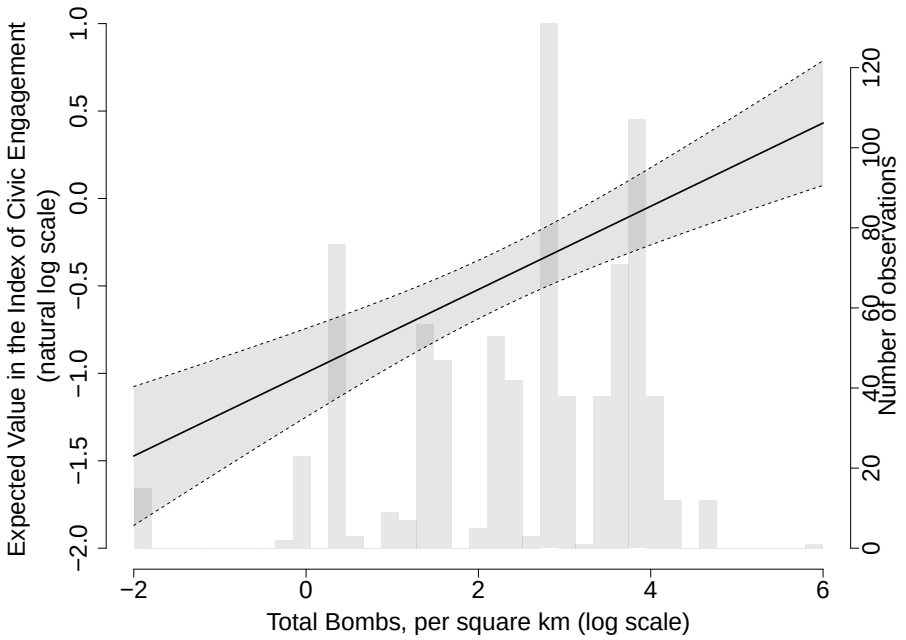


Figure 2.4: Civic Engagement (log scales)

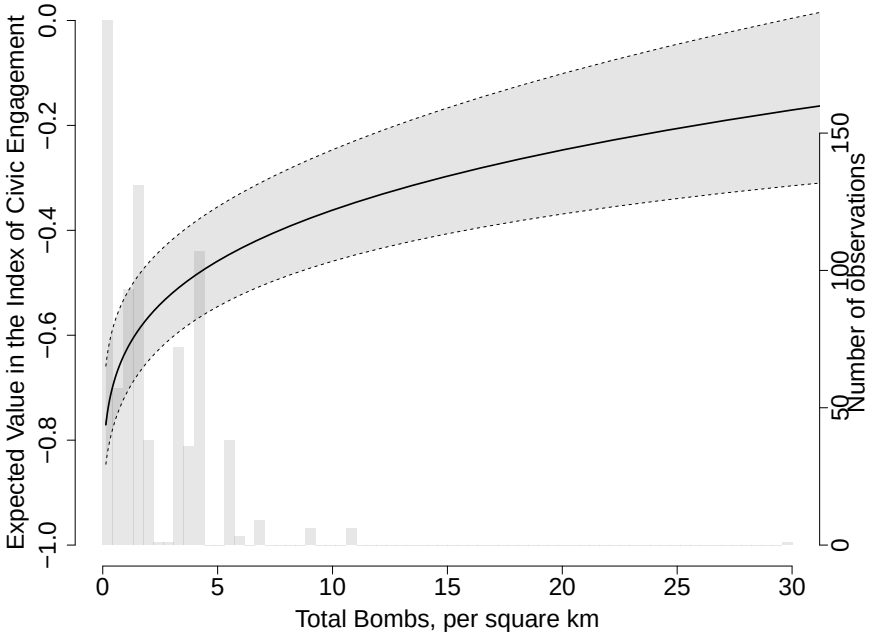


Figure 2.5: Civic Engagement (natural scales)

Note: Predicted values of civic engagement are generated from Model 4 in Table 2.1. They are generated by keeping all continuous control variables at their mean value and categorical control variables at their median values. Panel B only shows observations with a number of ordnance per square kilometer lower than 30. See the text for further details and Figure B.3 in the online Appendix for the same figure with an x-axis that ranges from its minimum to its maximum value.

Table 2.2: Robustness Checks

	Civic Engagement Index (log)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
(1) DV: Membership	0.04***	0.07***	0.12***	0.12***
	(0.01)	(0.02)	(0.02)	(0.02)
(2) DV: Excluding one item each time	0.07***	0.11***	0.10***	0.20***
(estimate of the most conservative exclusion)	(0.01)	(0.02)	(0.03)	(0.03)
(3) IV: Birthplace	0.08***	0.11***	0.12***	0.22***
	(0.01)	(0.02)	(0.02)	(0.03)
(4) Excluding members of parties	0.05***	0.06***	0.07***	0.13***
and labor organizations	(0.01)	(0.02)	(0.02)	(0.03)
(5) Controlling for membership of parties	0.06***	0.07***	0.08***	0.14***
and labor organizations	(0.01)	(0.02)	(0.02)	(0.03)
(6) Excluding Quang Tri	0.08***	0.12***	0.12***	0.24***
	(0.01)	(0.02)	(0.03)	(0.03)
(7) Excluding Top 20%	0.10***	0.12***	0.16***	0.22***
	(0.01)	(0.02)	(0.04)	(0.03)
(8) Excluding provinces	0.08***	0.11***	0.10***	0.22***
with ≤ 5 respondents	(0.01)	(0.02)	(0.03)	(0.03)
(9) North Vietnam only	0.15***	0.16***	0.21***	0.21***
	(0.02)	(0.02)	(0.03)	(0.02)
(10) Non-random Attrition	0.05***	0.10***	0.07***	0.20***
	(0.01)	(0.02)	(0.03)	(0.04)
Controls	N	Y	N	Y

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Cell entries refer to the unstandardized coefficients of the total bombs dropped in the province per square kilometer on civic engagement (log) in each different specification. Robust clustered-standard errors at the level of the province in parenthesis. See full models in the online Appendices B.7, B.8, B.9, B.10.1, B.10.2, B.11.1, B.11.3, B.11.2, B.13, and B.12, respectively.

of the skewness of the distribution of individual-level bombing, I further check the potential impact of influential observations in three ways: (1) by excluding respondents from the Quang Tri province, a province located right on the 17th parallel and the most heavily bombed of

all Vietnamese provinces;[67] (2) by excluding the top 20% most bombed observations; and, (3) by excluding observations from provinces with less than 5 respondents. Table 2.2 shows that the estimates are substantively identical.

A fourth source of concern is the pre-existing distribution of preferences before the beginning of the war. One could argue that the parallel runs through a particularly engaged population, and the observed high post-war engagement is not due to war exposure but to high pre-war engagement. First, the geographic distribution of the Vietnamese urbanization should lead us to believe that, in the absence of wars, those living around the major cities, around parallels 13 and 21, should be, if anything, more engaged than those living around the parallel 17 because of greater skills or opportunities (e.g., Oliver, 2000). And, second, most fighting between the French troops and the active guerrilla of Viet Minh (1945–1949) was located in northern areas of North Vietnam, where the Communist hubs were located.[68]Because the pre-war pro-Communist mobilization—before the parallel was drawn and the war began—was located far north of the 17th parallel, the hardest test for the main relationship is to focus only on evidence from North Vietnam.[69] Table 2.2 re-estimates the main models but using only respondents from North Vietnam. The results are substantively similar to those obtained from entire Vietnam.

A fifth concern is a potential bias from non-random attrition (or selection bias), which might be present if a non-random sub-sample of provinces had more war-time deaths, post-war deaths, or international out-migration after the war. Since those provinces that were more

[67]Note, however, that only 1 respondent is drawn from this province in the sample.

[68]See, for instance, (Lee, 1991, 167) and (Tucker, 2001, 643). In this regard, the *Declassified per Executive Order 13526, Section 3.3 NND Project Number: NND 63316. By: NWD Date: 2011* report that the first permanent Viet Minh bases were established in 1942–1943 in the northern areas of North Vietnam above Hanoi [B-26]. In these areas, after the people had been well organized, the first locally recruited guerrilla forces were formed under Viet Minh auspices [B-26]. As of the end of 1944, the Viet Minh claimed a membership of 500,000 in Vietnam and Tonkin (North Vietnam) was the region that embraced the largest number of them.

[69]Note that the use of respondents only from North Vietnam also allows to control for a potential effect of winning or losing the war, or change in regime type.

affected by the war are likely to have greater attrition, there are reasons to believe that attrition may be a source of bias.

To check the robustness of the estimates to non-random attrition, I place bounds on the treatment effect by generating new observations and imputing extreme values on their missing potential outcomes in the following manner. First, I use an exceedingly liberal estimate of the total number of people who are no longer in the country due to the war—a liberal estimate of attrition will lead to a conservative treatment effect. Though the most reasonable attrition rate is of 5.8% of the 2001 Vietnamese population, I check my findings to an implausibly large attrition rate of 10%.[70] Second, I generate artificially attrited observations equivalent to a 10% of the survey sample through a weighted re-sampling with replacement from the observed respondents. The weights are based on the value of bombings intensity. Thus, the likelihood that an observation from the original sample is re-used as an artificial observation in the final sample is proportional to the intensity of the conflict in the province of residence before 1975. Third, I assign the lowest possible value to the civic engagement to these re-sampled observations.[71] Table 2.2 reports a re-estimation of the analyses with the observed and missing observations in the same dataset. The last row shows that the consequences of attrition in the worst-case scenario exert little influence on the main coefficient in the models of civic engagement.

Finally, I relax the assumption of perfect exogeneity of the instrument. Table B.16 in the online Appendix shows the variation in the confidence intervals of the IV estimate with controls as a function of changes in δ—the parameter that violates the exclusion restriction.[72] The results show that for the estimates of conflict intensity on the civic engagement to lose

[70] See Appendix B.13 for further details on these estimates.

[71] If all missing observations would have had an extremely low value of engagement had they been in the sample, this would reduce the relationship between war exposure and engagement. Therefore, assigning extremely low values place a lower bound to the average treatment effect.

[72] The changes in the confidence intervals are based on an extension of the Anderson-Rubin test (Small, 2007). See the online Appendix B.14 for a brief technical overview.

significance at a 90% confidence interval, distance to the parallel needs to influence people's civic engagement by 0.18 standard deviations through a channel other than conflict intensity and the controls. This is about half of the total effect of distance to the 17th parallel on civic engagement, so it would require a massive violation of the exclusion restriction for the effect to lose its statistical significance.[73]

2.5.2 Exploring the Mechanisms

Why does war exposure lead to greater civic engagement? Section 2 proposed a psychological mechanism that might explain such a relationship. In this section, I empirically explore it and test the plausibility of alternative mechanisms.[74] Psychological explanations for the relationship between war exposure and participation in public life has to do with the long-term psychological imprint of respondents' exposure to war, whether direct or indirect, on a key trigger of participation: the value they place on participatory activities.

Table 2.3, Panel A and B, reports the effects of bombing on participatory values. Consistent with the PTG mechanism, those respondents who lived in areas that were more heavily affected by the war before 1975 are today more likely to prioritize values that emphasize the value of freedom of speech, and citizens' influence on how things are done in their communities and in governmental decision-making. The instrumental variable regressions

[73]The standardized regression coefficient of the distance to the parallel on civic engagement is -0.38 after adjusting for all other covariates.

[74]I do not conduct a formal causal mediation analysis because the sequential ignorability assumption is not plausible without making strong assumptions. For instance, the mediation effect might only be identified if we accept that treatment uptake (conflict exposure) is ignorable conditional on the distance to the border and pre-treatment covariates. Similarly, we should also accept that the mediator, i.e. participatory values, is ignorable conditional on the distance to the border and pre-treatment covariates. However, compliance with the treatment assignment is far from perfect and the mediators are influenced by unobserved confounders. Consequently, these are not reasonable assumptions and, thus, the causal analysis only allows for testing for a causal relationship between the treatment (conflict exposure) and the proposed mediator (which I treat as an outcome variable) but not for linking the mediator to the ultimate outcome. The following findings for mediators should be interpreted with caution as they may yield necessary, but not sufficient, evidence for the test of the causal mechanisms.

yield a substantively larger effect of war exposure on participatory values. Moreover, the online Appendix B.17 also shows that the evidence that conflict exposure is associated with participatory values is similar across its components.

Table 2.3: The Effect of Conflict Exposure on Participatory Values

	DV: Participatory Values (Index)			
PANEL A	Model A.1	Model A.2	Model A.3	Model A.4
Total Bombs, per km^2 (log)	0.01^{**}	0.04^{***}	0.03^{***}	0.06^{**}
(Residence, pre-1975)	(0.01)	(0.01)	(0.01)	(0.02)
	DV: More voice in community matters (Item 1)			
PANEL B	Model B.1	Model B.2	Model B.3	Model B.4
Total Bombs, per km^2 (log)	0.03^{**}	0.05^{***}	0.05^{***}	0.10^{***}
	(0.01)	(0.01)	(0.01)	(0.02)
	DV: More freedom of speech and voice in government (Item 2)			
PANEL C	Model C.1	Model C.2	Model C.3	Model C.4
Total Bombs, per km^2 (log)	0.02^{**}	0.05^{**}	0.02	0.07^{**}
	(0.01)	(0.01)	(0.02)	(0.02)
Estimation	OLS	OLS	IV	IV
Controls?	N	Y	N	Y
Observations	875	862	875	862
Provinces	48	47	48	47

The Index of Participatory Values is divided in two items. Item 1 is based on respondents' agreement to the following statement: Seeing that people have more say about how things are done at their jobs and in their communities. Item 2 is based on respondents' agreement to the following two statements: a) Giving people more say in important government decisions, and b) Protecting freedom of speech. See the full question wording in the online Appendix B.4.
Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model of Panel A, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province. See full model in Tables B.19 and B.20.

An alternative channel to the psychological theories comes from developmental theories of war. These posit that the association works through inter-regional variation in the post-conflict society, such as the difference in development or financial aid across regions, rather than individuals' direct exposure to the conflict. Two arguments have been proposed: (1) "development in reverse," and (2) standard neo-classical growth theory.

First, "development in reverse" suggests that war lowers the economic development of those areas most affected by the conflict, which reduces the quality of a region's social capital, organizations, and, thus, people's opportunities for civic engagement. The evidence presented here contradicts this argument, which means that it cannot serve as an alternative mechanism to explain the findings.

Second, standard neoclassical growth theories suggest that the destruction of physical infrastructure and the decrease in human capital accumulation during the war is followed by a greater post-war economic growth to return to pre-war levels of development. This would eventually lead to no long-run differences across regions. This is the case in Vietnam for which Miguel and Roland (2011) do not find any long-term evidence for inter-regional differences due to the bombing in measures of development, poverty, consumption levels, electricity infrastructure, or population density more than two decades after the end of the war. If such differences in inter-regional variation have actually taken place, then we might expect economic development to affect a number of societal outcomes, including civic engagement (Lipset, 1959).

Hence, I construct a measure that sums the total state investment per capita by province during 1976–1985 based on the Vietnamese Statistical Yearbook.[75] Table 2.4, Panel A, shows that state investments do not significantly increase civic engagement. Therefore, there is little evidence for a post-conflict development theory.

[75] Annual measures of total governmental investments per capita in each province is included in Miguel and Roland's (2011) dataset.

The post-conflict context may also be different for a number of non-economic reasons (e.g., norms). Post-conflict theorists would expect that, regardless of the place of residence during the conflict, those who have lived in a war-affected community during the post-conflict era should be more likely to be engaged in civic organizations. Hence, I exploit respondents' migration history from the survey to provide a general test of the post-conflict context hypothesis. Out of the 875 individuals that provide a place of residence before 1975, 868 also provide a province of residence in 2001. These 868 respondents can be matched to the province-level bombing data. From these, 123 respondents, or 14.1% of the sample with valid responses on both questions, changed their province of residence between 1975 and 2001. I use the bombing data linked to their historical residence and to their current residence to explore whether the place of post-war residence has an independent effect compared to the effect of the place of pre-war residence. Though none of the models can offer a valid causal estimate of the historical effect because they condition on a highly correlated post-treatment variable—the post-war place of residence—they do provide us with an estimate of the effects of bombing on people's current engagement through their post-conflict context.

Table 2.4, Panel B, tests this alternative hypothesis by regressing civic engagement on the difference of the bombings in the two provinces of residence—in 1990-2001 and pre-1975, respectively—adjusting for a baseline of bombings in the province of residence before 1975. Thus, this set of model specifications estimate the effect of deviations in the variable of bombing due to migration—the value of 1990-2001 periods with respect to the pre-1975—on a respondent's level of civic and political engagement net of the effect of bombing intensity in the province of residence in the pre-1975 period. The results presented in models 1 through 4 consistently show that changes in the bombing exposure of the province of residence due to migration do not change citizens' engagement, but all that matters is the place of residence before 1975.[76]

[76]Alternative specifications involve either restricting the sample to only those respondents who report a different province of residence in the 1990-2001 period with respect to the pre-1975, or including the value of bombings in the province of residence in the pre-1975 period and the value of bombing in the 1990-2001

Table 2.4: Alternative Mechanisms. The Effect of the Post-conflict Context and Personal Grievances

	Dependent variable: Civic Engagement (log)			
PANEL A: Post-Conflict Σ Investments	Model A.1	Model A.2		
Σ State Investments per capita, 1976-1985	−0.01	−0.01		
	(0.02)	(0.03)		
Observations	845	832		
Provinces	48	47		
PANEL B: Historical vs. Contemporaneous	Model B.1	Model B.2	Model B.3	Model B.4
Total Bombs, per km^2 (log): pre-1975	0.08***	0.12***	0.12***	0.25***
	(0.01)	(0.02)	(0.03)	(0.04)
Δ Total Bombs, per km^2 (log): 1975-2001	0.003	0.04	0.03	0.10**
	(0.04)	(0.05)	(0.03)	(0.05)
Difference in coefficients, p-value	< 0.02	0.06	< 0.01	< 0.01
Observations	868	857	868	857
Provinces	49	48	49	48
PANEL C: Personal grievances	Model C.1	Model C.2	Model C.3	Model C.4
Total Bombs, per km^2 (log): pre-1975	0.08***	0.12***	0.11***	0.22***
	(0.01)	(0.02)	(0.03)	(0.03)
Happiness and health controls?	Y	Y	Y	Y
Observations	868	857	868	857
Provinces	49	48	49	48
Controls?	N	Y	N	Y
Estimation	OLS	OLS	IV	IV

Note: *p<0.05; **p<0.01, ***p<0.001. Individual controls: age, gender, and education. Geographic controls: log of the population density of the province in 1960, average precipitation, average temperature, south dummy, latitude. In Panel C, each geographic control includes two variables in the model: its value in the pre-1975 province, and its value in 2001 minus its values in pre-1975. Instrumental Variable: Distance to the 17th parallel, pre-1975. Constants and control variables are omitted from the output. All models include heteroskedastic clustered-standard errors at the province level in parenthesis.

period in the same regression. Though both are inefficient modeling strategies, I implement both in the online Appendix. The substantive conclusions are unaltered (see B.24).

Finally, though the post-conflict context does not seem to drive the results, war exposure could generate individual grievances that encourage greater civic engagement. Though personal grievances are post-treatment—and so I do not include them in the main specifications—I construct two variables that capture these personal grievances: self-reported happiness and health. Table 2.4, Panel C, shows that adjusting for them does not alter the main finding.[77]

Taken together, the results from these empirical examinations provide little evidence for the external mechanism. Though not conclusive, the data from the Vietnamese war experience suggests that the most plausible channel through which wartime violence affects civic engagement is the individual level experience of violence rather than the post-war context.

2.6 Conclusion

This paper shows evidence that exposure to war increases civic engagement after more than a quarter century since the end of the conflict. Using bombing data from the Vietnam War and a representative public opinion survey conducted in 2001, I find a positive association between war exposure and participation in all kinds of civic organizations—regardless of its relation to the government.

To deal with the non-random geographic distribution of bombing, the empirical analysis exploits the arbitrarily drawn North-South Vietnamese border as an instrument for the effects of the Vietnamese bombing campaign. The two positive associations are robust to: (1) using an IV regression framework; (2) using respondents' birthplace—rather than pre-1975 province of residence—to construct the measure of war exposure; (3) excluding sub-samples of extreme observations; (4) accounting for non-random attrition rates due to deaths and international migrants; and, 5) violations of the exclusion restriction in the IV regressions.

[77] See online Appendix B.17.3 for further details on their measurement and the full models.

In addition, this paper offers suggestive evidence that these relationships may arise from an increase in people's participatory values. By contrast, there is little evidence that the war exposure-engagement link depends either on changes in the level of post-conflict development or other contemporaneous effects.

These novel findings contribute to the literature of the micro-level consequences of war, and to the understanding of the historical sources of political behavior in general. More specifically, these results show that the association between war exposure and civic engagement: (1) persists over long periods of time, at least, more than 25 years after the end of the conflict; (2) is causal; (3) generalizes to distinct types of wars such as proxy wars, which resemble more the historical Korean war or contemporaneous Syrian war; and, (4) might work through changing people's psychological makeup by moving political values related to the protection of societal freedom of speech, and people's say in community and governmental affairs up in one's life priorities, even in the context of a non-democratic regime like Vietnam.

These findings also contribute to our knowledge on the social, political, and economic effects of the Vietnam War. In the short-term, Dell and Querubin (2018) document that the U.S. bombings had significant effects on the security environment of bombed areas, weakened local institutions, and enhanced communist civic engagement, during the peak of the war (1969–1973). In the long-run, Miguel and Roland (2011) found no long-run effect of the bombings on Vietnamese regional differences in economic development. Hence, the evidence shown in this paper adds to this literature by showing that those who lived in bombed areas are more likely to be engaged in civic organizations, and hold more positive values toward freedom of speech.

While the Vietnam War generated plausibly exogenous variation in war exposure because of the arbitrary border, scholars should continue striving to combine exogenous variation

in conflict exposure and survey data to further explore the persistence of the micro-level consequences of war on people's attitudes and behaviors. An important next step for future research is to link micro-behavioral effect of war exposure to macro-political outcomes, including the maintenance of peace, changes toward regime democratization, the type of public policies, and the reduction or not of ethnic, social, and political divisions.

Chapter 3

Mobilizing Collective Memory: A Field Experiment on the Effects of Collective Threat on Voting Behavior

Abstract

Do messages of collective threat influence voting behavior? If so, which groups of people are more likely to be influenced by such messages? This paper presents the first estimate of the effect of collective threat messages on actual electoral outcomes in the context of an ethno-nationalist political conflict. This is done through a region-wide field experiment embedded in an organization's leafleting campaign in the 2017 Catalan regional elections in Spain. In precincts assigned to receive a *collective threat prime*, the vote share of Catalan pro-secessionist political parties increased by between 1% and 1.7% compared to zero-increase of precincts that received a vote encouragement with either no prime of collective threat or no prime at all. Building on research suggesting that wartime violence influences the formation of preferences across generations, I propose a resonance theory of mobilization in which messages of collective threat will be more influential among communities more severely affected by historical conflicts. The results show that the collective threat messages are particularly effective in those communities that were more heavily affected by the Spanish Civil War and the postwar repression, with an increase of between 3.6% and 5.8% in the support for Catalan pro-secessionist parties. These findings have far-reaching implications for our understanding of how conflict history combined with the contemporary rhetoric of collective threat may enhance nationalist-secessionist mobilization.

Various forms of cultural, economic, and physical external threats have become dominant political themes across liberal democracies. Common culprits are immigration, globalization, and elites. Certain actors such as populist, nationalist, and radical right movements seem to do particularly well amidst climates of threat. In this context, political elites often heighten people's sense of collective threat to garner political support.[78] However, it remains unclear whether these parties gain support because their messages are effective; or because they simply become more appealing to voters in a political climate of insecurity. In other words, do messages of collective threat influence voting behavior? If so, which groups of people are more likely to be swayed by such messages?

A number of scholars point to the role of leaders' threat messages in mobilizing citizens for group violence, mass mobilization, and political persuasion (Collier et al., 2003; Petersen, 2002). Even though useful for building theory, the evidence for the impact of threat messages remains limited. In this regard, Collier and Sambanis (2003, 322) admitted that while "many case studies of ethnic wars point to the importance of political leadership in mobilizing support for violence, [...] quantitative studies have been unable to test the effects of leadership." Some recent efforts have tried to fill this gap. However, scholars have not been able to go beyond observational evidence or survey experiments, and have only yielded mixed findings (e.g., Horowitz and Klaus, 2018; Mutz, 2018; Selb and Munzert, 2018). Therefore, we lack a systematic evaluation of whether collective threat appeals may *cause* a change in political behavior in *real-world settings*.

Moreover, comparative politics scholars acknowledge that historical events leave long-lasting legacies on contemporary behaviors. One of the major mechanisms has to do with the

[78] I define collective threat messages as fear-based appeals that highlight the potential of a hostile action on someone or a community by an external force.

shift in people's socialization processes (e.g., individual, family, peers, and local environments)(Acharya, Blackwell and Sen, 2016; Lupu and Peisakhin, 2017; Nunn and Wantchekon, 2011; Rozenas, Schutte and Zhukov, 2017). Hence, I suggest a *resonance* hypothesis of political behavior by which political messages are more credible and vivid—hence more effective in shaping voting behavior— if they *resonate* with prior socialization processes.

This paper empirically offers an evaluation of the impact of appeals to collective threat on voting behavior. In the context of the nationalists/secessionist conflict in the Spanish region of Catalonia, I test whether collective threat messages: 1) influence the support for nationalist/secessionist parties; and, 2) increase the vote of nationalist/secessionist parties especially among those communities that were more heavily affected by the Spanish Civil War and the ensuing Francoist repression (1936–1945).

I study these two questions through a unique clustered randomized controlled trial and novel historical data on the hotspots of the wartime violence during the 1936–1945 period. First, I present the results of a precinct-level region-wide experiment embedded in a partisan non-governmental organization's campaign in the 2017 Catalan legislative elections using administrative records. Between December 18 and 19—two and three days before the elections—an estimated 37,000 leaflets were left across 98 villages, with a total population of 80,000 inhabitants. In total, 140 villages were assigned to one of three following groups: a treatment group (63 villages), a placebo group (35 villages), and a control group (42 villages). The majority of households within a municipality in the treatment group received a *collective threat* leaflet, which encouraged them to vote in favor of Catalan pro-independence by reminding voters about a recent episode of police brutality by the Spanish police forces. The majority of households within the municipalities in the *placebo* leaflet received the same encouragement to vote in favor of Catalan pro-independence parties, but with no reference to any threatening event. Finally, villages in the *control group* did not receive any leaflet at all.

All results are based on official election outcomes at the level of the village. I find it increased Catalan pro-secession parties' vote share in villages allocated to the *collective threat* leaflet by between 1% and 1.7% compared to similar villages assigned to the placebo and control groups. The estimate corrects for imperfect compliance in the delivery company, and are precisely estimated. Moreover, the results also suggest that the vote shares of Catalan pro-independence parties rise at the expense of the vote shares of neutral parties— but not unionist parties. This means that not only threat messages increase support for nationalist/secessionist parties, but also fuel local-level political polarization.

Second, I test the resonance hypothesis. For this, I evaluate the heterogeneity of the effect of the *collective threat treatment* across villages' historical experiences with the Spanish Civil War and the ensuing Francoist repression (1936–1945). Historical violence intensively, yet heterogeneously, affected Catalonia. During and after the war, the Francoist regime and supporters often employed individual or mass extra-judicial executions against dissidents—leftist supporters and regional nationalists—and most were buried in nearby mass graves. Thus, I use the geographic information and the estimated size of about 2,650 mass graves scattered throughout Spain to construct a local-level indicator of the intensity of the wartime violence. Then, I test the resonance model by evaluating the interaction effect of the collective threat treatment from the field experiment with the indicator of historical violence.

I find a significant increased effect of the *collective threat treatment* among those Catalan communities that were more severely impacted by the Spanish Civil War and the Francoist repression. Specifically, while the treatment effect on the support for Catalan pro-secessionist political forces means an increase of between 1.4% and 2.4% in those villages with an average history of wartime violence and state repression, the support for nationalist/separatist political forces increased by between 3.6% and 5.8% in those villages that are one standard deviation above the mean in the value of historical exposure to violence. These findings have far-reaching

implications for our understanding of how conflict history combined with the contemporary rhetoric of collective threat may enhance nationalist-secessionist mobilization and political polarization.

3.1 Campaigns and Mobilization in Divided Societies

The dynamics of public-elite interactions have been the focus of much political science research since the behavioral revolution in the 1940s. Still today, scholars heatedly debate whether voters' choices are susceptible to influence from political elites.

Despite the enormous emphasis that political pundits assign to the role of elites in shifting public opinion, early scholars in the U.S. contended that the impact of elite communication on voters is negligible. These studies portrayed partisan and voting preferences as originating from long-term socialization processes, and unmoved by electoral cycles (Berelson, Lazarsfeld and McPhee, 1954; Campbell et al., 1960; Finkel, 1993). As individuals avoid attitude-discrepant information, some scholars argue that there is little opportunity for exposure to new information to have any remarkable effect on behavior (Stroud, 2008). Thus, Brady and Johnston (2006) conclude that the dominant view is that campaigns have "minimal effects" on voters.

The last generation of campaign research employs field experiments to provide credible causal estimates of their interventions.[79] In this regard, Gerber and Green's (2000) landmark work

[79]Note that some observational studies have found strong campaign effects (e.g., Ansolabehere, Iyengar and Simon, 1999; Baum, 2002). However, these estimates may be subject to several shortcomings (Arceneaux, 2010). For instance, some of them use self-reported measures of campaign exposure, which may lead to biased estimates due to their systematic association with important confounders (Vavreck et al., 2007). While others overcome these limitations by using objective measures of exposure to campaign stimuli, exposure to messages tends to be correlated with individuals' prior beliefs either because some people decide to pay attention to specific political elites and their messages (Stroud, 2008) or because elites choose to reach out some individuals and not others (Wielhouwer, 2003).

led to an explosion of get-out-the-vote field experiments on voter turnout.[80] Despite the emphasis on turnout, scholars have paid relatively little attention to effects of campaigns on vote choice. To a large extent, this is the result of the secret ballot. While voter turnout is recorded and made publicly available at the level of individuals in many countries, individuals' vote choice is secret. Scholars usually turn to posttreatment surveys to approximate their outcomes of interest by including questions on vote intentions or vote recall (Arceneaux, 2007; Arceneaux and Nickerson, 2009; Bailey, Hopkins and Rogers, 2016; Barton, Castillo and Petrie, 2014; Cardy, 2005; Dewan, Humphreys and Rubenson, 2014). The results from these experiments are mixed and, in a recent meta-analysis, Kalla and Broockman (2018) assert that "the best estimate for the persuasive effects of campaign contact and advertising on Americans' candidate choices in general elections is zero."

The minimal effects of most real-world interventions do not imply that public opinion and vote choice cannot be rapidly changed by political entrepreneurs. It simply reveals that few scholars have conducted theory-driven interventions with convincing empirical support. In the field of group relations, theory-driven work show remarkable exceptions to the null findings. For instance, Green and Paluck's (2009) and Paluck's (2009) intensive field experiment in Rwanda convincingly showed that listening to a radio show designed to promote reconciliation led to shifts in key social beliefs, including social norms, social trust, empathy, and willingness to cooperate with others.[81] Building on social psychology theories of perspective-taking (Todd and Galinsky, 2014), two recent interventions also show how attitudes can be quickly changed. In this regard, Kalla and Broockman (2018) provide evidence suggesting that intensive canvassing with 10-minute conversations that encourage people to take the perspective of others can reduce transphobia. Similarly, Simonovits, Kézdi and Kardos (2018) employ an

[80] For an extensive review, see Green and Gerber (2015); Gerber and Green (2017).
[81] Although not listeners' personal beliefs.

online perspective-taking game to demonstrate that Hungarians who play the role of a Roma immigrant reduce prejudice against members of the Roma community.

Therefore, despite the fact that most campaign interventions on persuasion based on the electoral research in the U.S. have found minimal effects, there is evidence that theory-driven interventions have the potential to challenge the general stability of political predispositions and opinions. I now turn to a discussion of why collective threat messagess may be particularly effective at bringing about a change in political behavior in divided societies.

3.1.1 Collective Threat Messages and Political Behavior

In this section, I build a theoretical framework to generate empirical hypotheses about the effects of priming collective or group threat on vote choice and political polarization. Here, I conceptualize primes of collective threat as fear-based appeals that highlight an intention to inflict pain, injury, damage, or other hostile actions on an individual or the community by an external force.[82] For this, I draw on behavioral theories of persuasion, emotions, and group dynamics.

To begin with, political messages may constitute important sources of opinion change. A psychological model of framing suggests that issues and decision-making processes can be viewed from multiple perspectives, each associated with a set of considerations (e.g., Chong and Druckman, 2007*b*,*a*; Druckman, 2004). In this framework, individuals place different weights or emphases on various considerations. The process by which people reorient their opinion, decision, thinking, or conceptualization by shifting the weights associated with each consideration is generally referred to as *framing*.

[82]Note that appeals of collective threat are conceptually distinct from messages of group clientelism (Wantchekon, 2003).

Abundant empirical evidence suggests that external messages that emphasize a specific issue while ignoring others affect people's political judgment by activating this process (e.g., Chong and Druckman, 2007*a*; Druckman, 2004). However, not all frames are created equal. A long line of researchers point to messages that trigger emotions, such as anger and anxiety, as particularly effective at changing political attitudes and behaviors (Brader, 2005, 2006; Brader, Valentino and Suhay, 2008; Claassen, 2016; Mackie and Smith, 2002; Marcus, Neuman and MacKuen, 2000). While messages associated with positive emotions serve to strengthen existing loyalties, messages associated with negative emotions produce vigilant attitudes, which increase the chances of change in opinion (Brader, 2005).

In the context of divided societies, scholars point to the profitability of appealing to group identities, an affectively-charged political attachment, for mobilizing the support of in-group members (Chandra, 2007; Horowitz, 1985; Schmidt and Spies, 2014; Wilkinson, 2006). In this regard, societal actors often attempt to strengthen (or weaken) group identities and collective grievances to garner electoral support and build political coalitions (Petersen, 2002; Posner, 2004; Wilkinson, 2006). This, in fact, could explain why identity attachments surge around election times (Eifert, Miguel and Posner, 2010), which, in conflict-prone settings, may increase the chance of violent outbreaks (Dunning, 2011; Wilkinson, 2006).

In this process of eliciting collective attachments, emotions are powerful tools. A variety of scholarly explanations center around the role of emotions–especially negative emotions such as fear, anger, resentment, and hatred– in explaining how both the elites and the masses make behavioral decisions (De Figueiredo and Weingast, 1999; McDoom, 2012; Horowitz, 1985; Petersen, 2002; Wilkinson, 2006). Hence, the two major scholarly traditions of group dynamics, rationalists and affective theories, put emotions at the core of their theories—although they do so with different degrees of intensity.

For rationalist theorists, elites' sense of existential threat increases their chances to "gamble for resurrection" by taking risky decisions, such as engaging in inter-group violence, taking more radical stances with other groups, and leading a secessionist path to maintain their position (De Figueiredo and Weingast, 1999; Lake and Rothchild, 1996; Posen, 1993).[83] Emotions are even more fundamental for affective theories of group dynamics. For these, emotions not only cause the elite and masses' rational reactions but also define their response. For Horowitz (1985), group anxiety results from inter-group comparisons, and produces "extreme reactions to modest threats" (179). While existential threat is the most extreme form, threat can still be a powerful motivator even if it simply affects group's cultural identity, its rights, or its self-worth. Similarly, Kaufman's symbolic politics theory suggests that both structural factors–an opportunity for collective action–and emotional factors–ethnic fears—interact to drive inter-group dynamics. Finally, Ross' (2007) psycho-cultural narratives argue that "physical attacks on their group" and/or "symbolic attacks on its identity" produce group threats responsible for increased animosity and polarization between groups (37).

Drawing on the above theoretical discussion, we should expect that priming a contemporary attack from an external force on one's community, especially if this attack involves a forceful action, should be perceived as a collective or group threat. In turn, the increased threat should heighten perceptions of being under attack, be it physically or symbolically, which should lead to greater animosity toward those individuals, organizations, institutions, and political forces that provide support to the threatening action. This reasoning leads to the following two hypotheses:

Hypothesis 1: *Campaign appeals that heighten perceptions of collective threat positively influences their vote share.*

[83] In this conception, a sense of threat may arise as a result of information failures between the parties or skepticism toward credible commitment from members of the other groups (Lake and Rothchild, 1996; Posen, 1993).

Hypothesis 2: *Campaign appeals that heighten perceptions of collective threat can significantly increase parties' political polarization.*

3.1.2 A Resonance Model of Framing Collective Threat

Scholars of comparative politics acknowledge that historical events leave long-lasting legacies on the formation of political preferences and behaviors.[84] In the presence of strong identity-based cleavages, such as ethnic, religious, or linguistic divisions, a record of historical political violence seem to shift people's psychological make-up, family and local environments in such a way that strengthen attachments to identity groups (Balcells, 2012, 2017; Lupu and Peisakhin, 2017), affinity with in-group leaders, support for in-group political forces (Cassar, Grosjean and Whitt, 2013; Hadzic, Carlson and Tavits, 2017; Rozenas, Schutte and Zhukov, 2017), and attitudes toward the out-group (Beber, Roessler and Scacco, 2014; Berrebi and Klor, 2008; Getmansky and Zeitzoff, 2014). Contributing to this scholarship, I contend that conflict experiences should increase people's susceptibility to appeals to collective security threats.

As in the psychology literature (Higgins, 1996; Price, Tewksbury and Powers, 1997), framing in political science has been conceptualized as a three-step process involving availability, accessibility, and applicability (Chong and Druckman, 2007*a*). Considerations are effective if they have first been stored in long-term memory, so that they can become *available* for retrieval (Higgins, 1996). Additionally, considerations must be *accessible*. External cues, salient events, or their reminders, are required for available considerations to be useful. Finally, a consideration must be deemed as appropriate or *applicable* to a specific case to

[84]For more information on the long-term effects of important events on political behavior, see applications on wars (Lupu and Peisakhin, 2017; Rozenas, Schutte and Zhukov, 2017), slavery (Acharya, Blackwell and Sen, 2016; Nunn and Wantchekon, 2011), and regime transitions (Barceló and Muraoka, 2019; Pop-Eleches and Tucker, 2017).

eventually be retrieved and used in constructing new attitudes and behaviors (Strack, Martin and Schwarz, 1988).

Building on this framework, I suggest a *resonance hypothesis* to help explain which types of communities may be more likely to mobilize. Specifically, I argue that individuals who personally experienced, or have been socialized in a context of, a significant record of political violence are more susceptible to cues of collective threat.[85] Considerations associated with collective threats are more available to people who live in conflict-affected areas, and these are more readily accessible for retrieval from their long-term memory when making new political judgments. Thus, fear-based campaign appeals can be particularly successful among post-conflict communities because these appeals revive deep-seated memories of past experiences that remain available and accessible in the back of people's mind. Overall, messages of collective threat are more credible, more vivid and, consequently, more likely to be available for retrieval in forging new opinions and behaviors in post-conflict settings. Therefore, when campaign messages of collective threats become equally available to all communities, their considerations should be more accessible—and, thus, more effective at changing behavior— to those who live in areas more severely impacted by political violence. This behavioral model leads to the following hypothesis:

Hypothesis 3: *Campaign appeals that elicit perceptions of collective threat influence parties' vote share especially among communities with a record of historical political violence.*

[85]Obviously, memories of past experiences do not only form through personal experiences (e.g., direct exposure with violence), but also through family and peers (e.g., stories of victimization by relatives or acquaintances), as well as contextual socialization (e.g., memorials, museums).

3.2 Setting: A Clustered RCT in Catalonia

Our understanding of the impact of collective threat appeals and their interaction with prior experiences on voting behavior is mostly limited to qualitative evidence. This is understandable for several reasons. First, political elites and organizations endogenously target appeals of collective threats to potential supporters (Hillygus and Shields, 2014). Second, most conflict-prone settings are found in developing societies where group animosity may lead to violent tactics (Dunning, 2011; Wilkinson, 2006), which limits our ability to randomly assign primes of collective threat and remain within ethical boundaries. Moreover, even if an experiment were possible, randomization would not eliminate traditional survey and survey experiment biases. In phone surveys, questions of political behavior are especially prone to misreporting (Campbell, 2010), and some key questions such as vote choice have response rates even lower than 20 percent in advanced democracies (Barton, Castillo and Petrie, 2014). Similarly, survey experiments ensure that the treatments are assigned randomly but cannot ensure that the intensity of the treatment and the setting of exposure are comparable to real-world campaign messages (e.g., Barabas and Jerit, 2010). For all these reasons, we still lack a convincing causal test of real-world primes of collective threats on actual behavioral outcomes.

To overcome these limitations, I focus on the conflict in the Spanish region of Catalonia, as it provides an ideal setting to implement a randomized controlled trial (RCT) to test the causal effect of collective threat appeals, and its interaction with history, on actual electoral outcomes. First, the Catalan conflict has a group dynamic but in the context of an advanced democracy, which also implies that there is a negligible risk of group or communal violence.[86]

[86]No records of relevant group or communal violence have been recorded since the beginning of the democratic regime in 1978.

The conflict is channeled through democratic means, i.e., demonstrations, strikes, and with elections.

Second, while Catalan politics have been typically depicted as bidimensional, socioeconomic and identity-based cleavages (so-called ethno-linguistic or territorial cleavage), the latter cleavage has come to dominate Catalan politics in the last years (e.g., elite messages, media attention, and governmental coalitions) (Rico and Liñeira, 2014). In this context, political parties have been forced to take clear stances on the territorial issue: while regional parties have united to favor independence, country-wide political parties have supported the unity (Rico and Liñeira, 2014). The clarity of parties' positions facilitates the outcome measure with regards to which parties support the independence, which do not, and which stand neutral or undefined.

Third, even though the Catalan society is roughly divided in two halves along the identity dimension, Spanish-born and/or Spanish-speakers tend to be geographically clustered around major urban centers such as Barcelona, while Catalan-born and/or Catalan-speakers predominate in rural areas (Barceló, 2014; Lepič, 2017; Rodon and Guinjoan, 2018). This geographic distribution facilitates the implementation of the field experiment because many rural areas remain homogeneous, with a strong Catalan ethno-linguistic majority. Among these communities, reminders of the widespread forceful actions against Catalan protesters on 1 October 2017 at the hands of the Spanish violence elicits a heightened perception of collective or group threat.

The exceptional situation of the 2017 Catalan regional elections provides an excellent setting to test the effects of campaign appeals on vote choice and polarization. The elections to the Catalan Parliament took place on 21 December 2017. On 1 October, the Catalan government organized a referendum on independence, which the Constitutional Court considered illegal.

Nearly 2.3 million voters participated in the elections and over 90% voted in favor of independence. Spanish security forces[87] — Guardia Civil and Policia Nacional — tried to prevent the vote with riot squads who used batons, rubber bullets, and tear gas canisters. The Catalan Health Department reported about 900 injured people (CatSalut, 2017). On 27 October, the Parliament of Catalonia declared an independent state in the form of a republic. For the first time, the Spanish government activated a constitutional provision to dismiss the members of the regional parliament and government, suspend the autonomy, and call for new elections. This electoral context allowed this research to generate a realistic message of *collective threat* embedded in the electoral campaign of the regional elections.

Finally, the historical context of Catalonia makes it an excellent setting to evaluate the resonance model. The Spanish Civil War and the ensuing Francoist repression (1936–1945) heavily, yet heterogeneously, affected Catalonia. The military revolt supported by conservative and traditionalist ideological factions against Spain's Republican government in 1936 led to the Spanish Civil War (1936–39). The territory divided between the areas controlled by those who remained loyal to the Republic government – the Republicans – and those who supported the revolt – the Nationalists. During the Spanish Civil War, individual or mass extra-judicial executions were common in the front-line but also in the rear-guard to neutralize political dissidence within each of the controlled areas. With the victory of the Nationalists in 1939, Francisco Franco established a military dictatorship (1939–1975). Massive state repression against political dissidents, including loyalists to the Republic, supporters of leftist groups, and regional nationalists from Galicia, Catalonia, and the Basque Country, characterized the early years of Francoist Spain (1939–1945).

[87]The Catalan police — the *Mossos d'Esquadra* — did not participate in the use of force action against voters.

3.3 Research Design

The research design presents a cluster randomization controlled trial in which a collective threat is elicited among Catalan communities through a leaflet campaign within a three-day window prior to the regional elections in 2017 Catalonia. In this section, I discuss the measures of the outcomes and the historical wartime violence before I describe the experimental intervention, including the pre-randomization adjustments and the randomization rule, present some randomization checks and, finally, the estimation strategies.

3.3.1 Data and Measures

I have built an original dataset with the 947 Catalan municipalities as the units of analysis. This dataset includes measures of support for Catalan pro-secessionist parties, neutral, and constitutionalist parties, other demographic characteristics, the geographic distribution of the field experiment that primes the contemporary source of collective threat; other village characteristics such as distance to province capital, province, percentage of Catalan-born population, population size, and population density.

Measures of Vote Choice

The dataset includes measures that capture the support for pro-independence, neutral, and unionist parties. I operationalize the support for pro-independence parties as the sum of the vote shares of Junts pel Sí and *CUP-Crida Constituent* in 2015 and *Junts per Catalunya*, *Esquerra Republicana de Catalunya*, and *CUP-Crida Constituent* in the 2017 regional elections. The support for pro-unionist parties are measured as the sum of the vote percentages of *Ciutadans-Partido de la Ciudadanía*, *Partit dels Socialistes de Catalunya*, and *Partit Popular*.

Finally, neutral parties are measured as those that are neither pro-independence nor pro-union, which has been the *Catalunya Sí que es pot* in the 2015 regional elections and *Catalunya en Comú–Podem* in the 2017 regional elections.[88]

Measuring Historical Wartime Violence and Repression

Many of the bodies of those who died during either the war or the post-war Francoist political repression remain in mass graves scattered throughout Spain. The geographic location of mass graves indicates the wartime violence hotspots during the Civil War and the ensuing Francoist repression because most bodies were buried nearby the killing locations. I have built a dataset with information of 2,650 mass graves in Spain to create a measure of the local experience of war-related violence (1936–1945).[89] This dataset includes information such as the location of the mass grave, the number of buried bodies, the number of identified and unidentified bodies in the grave, whether the grave has been exhumated or not, and the estimated date of the grave.

I combine information about the location and size of mass graves to create an indicator of the log of the number of bodies buried in each municipality. First, I create a marked point pattern object with the location of the graves using the coordinates and the size of the graves as the markers of the object.[90] Because the local-level exposure to wartime events depends on the distance to those events, I perform a spatial smoothing using inverse distance weighing of the observed mass graves. In brief, if mass graves that are located at $l_1, \ldots, l_n$ have the size of $m_1, \ldots, m_n$, then the smoothed value at a location u is given by: $g(u) = \frac{\sum_i w_i l_i}{\sum_i w_i}$ where the

[88]See Medeiros, Gauvin and Chhim (2015), Rico and Liñeira (2014), or Orriols and Rodon (2016), for an extensive description of the position of political parties along the identity cleavage in Catalonia.

[89]A Python script has been used to automatically collect all available information from the directory of mass graves made available by Spain's Ministry of Justice, and available at `http://www.memoriahistorica.gob.es/es-es/mapafosas/Paginas/index.aspx`. Accessed on February 10–15, 2018.

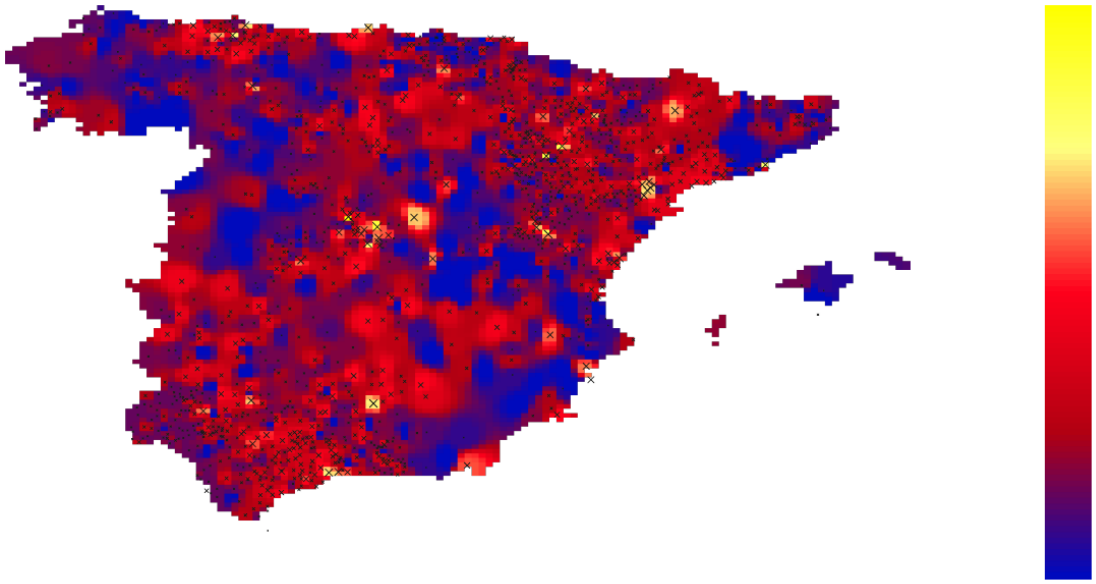
[90]I add up those mass graves that share their geolocation. I also restrict the analysis to continental Spain and Balearic Islands and, thus, I exclude the mass graves in Canary Islands. There are 1,532 unique locations in the point pattern object.

weights are the inverse p-th powers of distance, $w_i = \frac{1}{d(u,x_i)^p}$, where $d(u, x_i) = ||u - x_i||$ is the euclidean distance from u to x_i. The smoothed values of mass graves are calculated at a grid of 128×128 pixels, each pixel with a size of 9.67km×7.49km. The top of Figure 3.1 maps the inverse distance weighing interpolation for the pixels in Spain with marks that indicate the location of the mass graves (the size of the marks are proportional to the size of the mass graves). Then, I assign the value of the pixel which minimizes the distances between the centroids of the pixel and the municipality. The bottom of Figure 3.1 maps the interpolated value of the size of mass grave for each municipality in the region of Catalonia.[91]

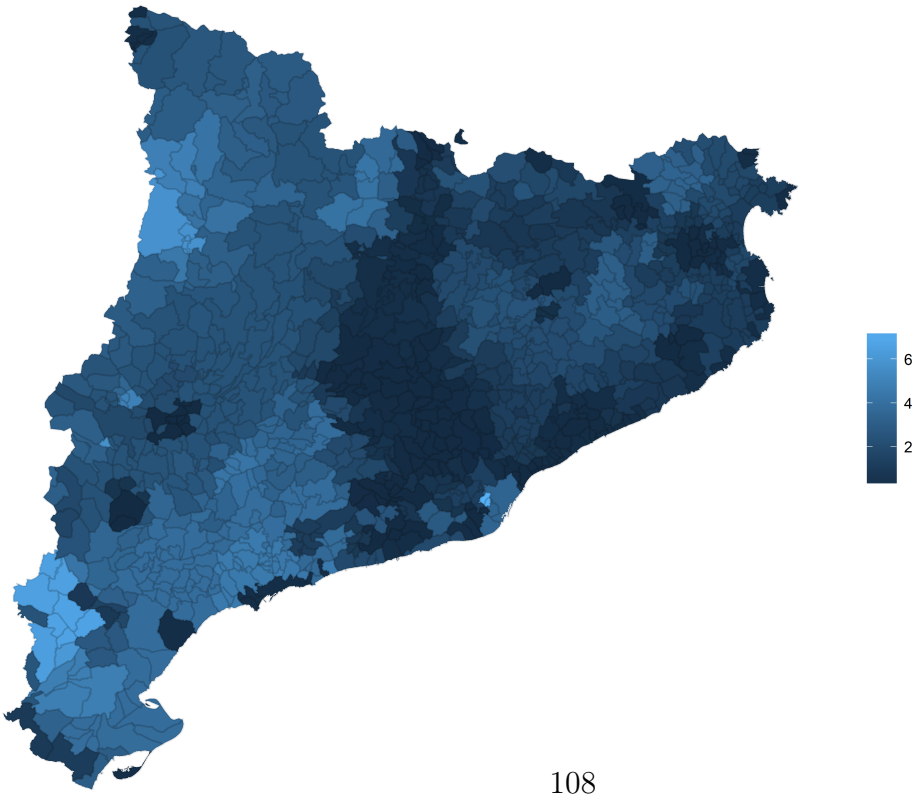
[91]The map of Catalan municipalities is a subset of the map for all Spanish municipalities. See the online Appendix C.4 for all Spanish municipalities.

Figure 3.1: Hotspots of Historical Wartime Violence

a) IDW interpolation (in pixels), Spain



b) Interpolated values, Catalonia



3.3.2 The Experimental Intervention

The *Assemblea Nacional de Catalunya*, a Catalan NGO that seeks political independence of Catalonia from Spain through peaceful means, conducted a campaign to promote the vote for pro-independence parties in the days leading to the 2017 Catalan elections.[92] The design of the intervention aligned with the political objectives of the organization.[93]

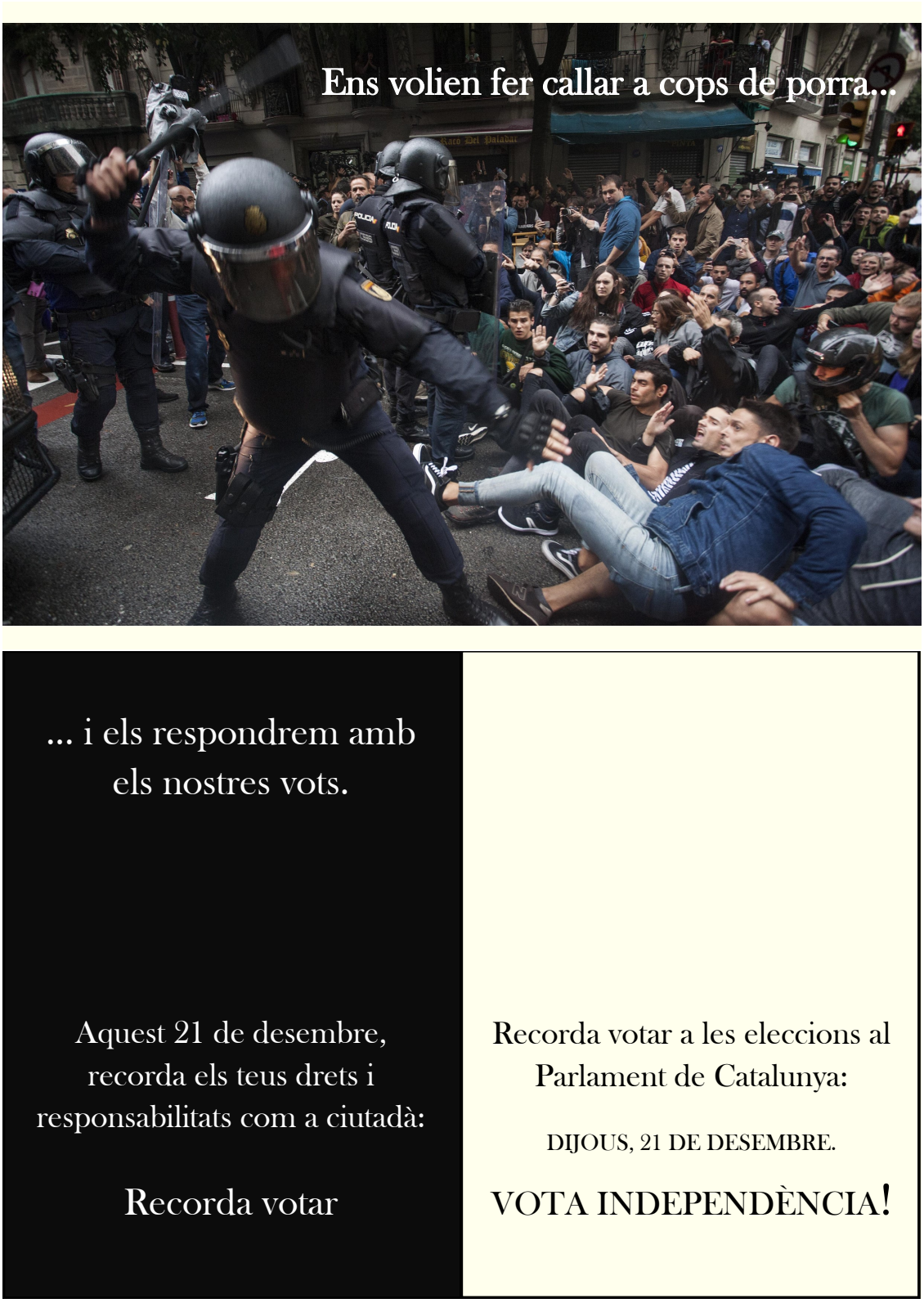
The campaign was designed to induce communities to vote for pro-independence parties through reminders of a recent episode of out-group aggression. Figure 3.2 shows the treatment flyer. The upper image is the front of the flyer, the lower image is the back of the flyer. The front of the flyer includes an iconic image taken in lÉixample, Barcelona, on October 1, and the sentence: "They wanted to silence us with their batons."[94] The back of the flyer continues the sentence as "and we will respond to them with our ballots. On December 21, remember your rights and duties as a citizen: Remember to vote" [back side, left]. "Remember to vote in the elections to the Parliament of Catalonia: Thursday, December 21. Vote Independence! [back side, right]." By contrast, the placebo letter contains no information related to the state violence, and simply encourages a vote in favor of pro-independence parties. The placebo message simply states: "Express ourselves with our ballots. On December 21, remember your rights and duties as a citizen: Remember to vote [Back side, left]. Remember to vote in the elections to the Parliament of Catalonia: Thursday, December 21. Vote Independence! [Back side, right]."

[92]The organization has 80,000 members and a budget over 5 million euros (most of the budget is devoted to campaigning and advertising). Financial resources are not obtained from public aid, but from private donations and membership fees. Though it has clear political objectives, the organization has no official ties with any government.

[93]See the online Appendix C.1 for a description of the ethical considerations of this project.

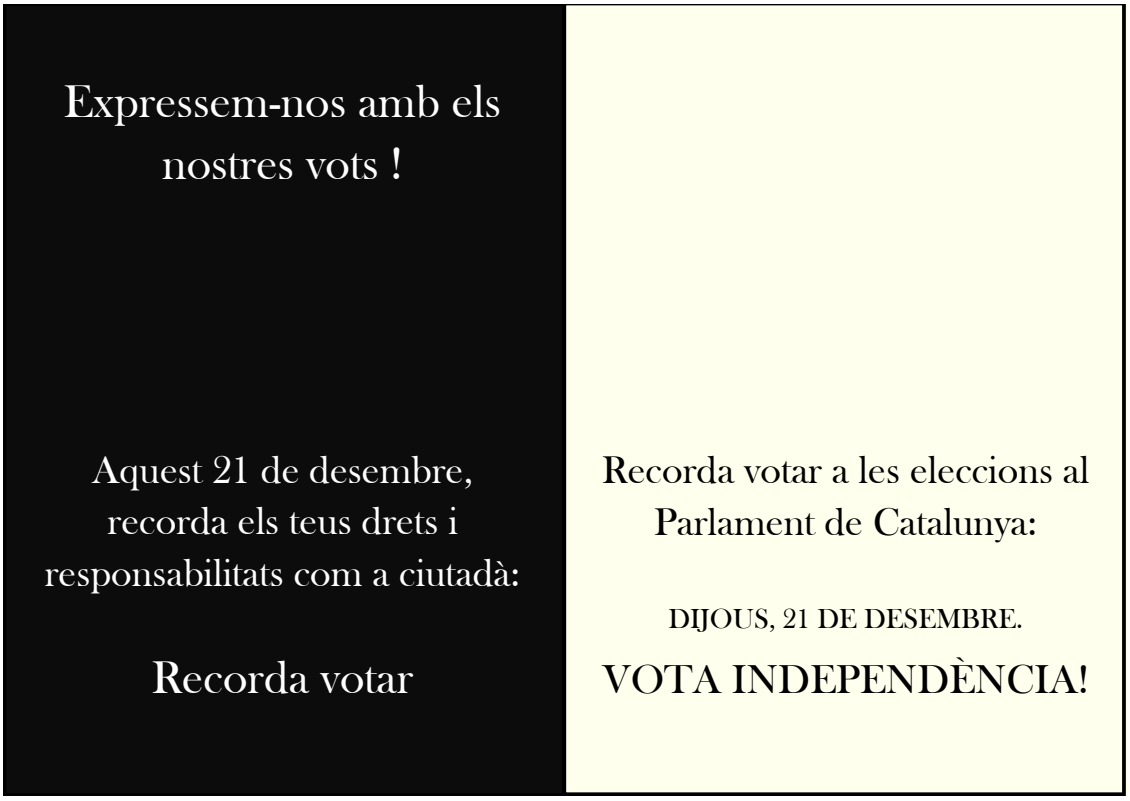
[94]Explicit permission to use this image for this project's purposes was given by Robert Bonet, the image's author.

Figure 3.2: Treatment Flyer



Notes: The upper image is the front of the flyer, the lower image is the back of the flyer.

Figure 3.3: Placebo Flyer



Notes: The image is the front of the placebo flyer.

The campaign was implemented during a two-day period, beginning three days before the election day, on December 18–19, 2017. The distribution of 37,000 flyers attempted to reach 98 small villages in Catalonia. Due to the geographically scattered location of the enumeration areas, a private company was hired for the door-to-door leaflet distribution services. The delivery was intense in the urban nucleus of villages. However, farms and isolated houses were excluded from the delivery areas. Because between 5 and 7 vans provided simultaneous services, campaign activities were largely contemporaneous in all counties. I discuss the randomization level, the sampling, other data sources, some problems of implementation, and other non-compliance issues in detail below.

3.3.3 Randomization Level

The campaign implements a cluster randomization controlled trial in which villages as a whole are randomized. Thus, all households within a given village are assigned to the same study arm.

There are several advantages to employing this technique. First, cluster randomization minimizes the spillover of treatment effects from one network to another. An emerging trend in political science shows that network structure crucially impacts preference formation (e.g., Druckman, Levendusky and McLain, 2018; Klar, 2014; Klar and Shmargad, 2017; Sinclair, 2012). Network members such as family, friends, peers, and neighbors shape political attitudes and behavior. When treatments (campaign messages) spread across units of different study arms through shared networks, causal inferences become biased. An advantage of the cluster randomization at the level of the municipality is that networks are more likely to be independent across study arms and, thus, spillover biases are less of a concern.

Second, municipality-level cluster randomization focuses on the effectiveness of the treatment under actual conditions of exposure. Spillovers not only are less of a concern, but also they are naturally accounted for in the treatment estimate. Government, organizations, and political entrepreneurs are generally interested in bringing about behavioral changes in an aggregate of people living together in a community, be it a neighborhood, a town, or an entire country, and less often in behavioral changes in particular individuals. Therefore, cluster randomization allows for a treatment to spread within local networks.

In evaluating the impact of a treatment on vote choice, individual randomization must rely on self-reported behavior measured in post-experimental surveys due to confidentiality issues. While not always a concern, post-experimental surveys share common biases in self-reported

information such as social desirability and recall bias. Even if these biases are balanced across study arms, if the treatment influences norms about socially desirable behaviors, then reported or recalled behaviors across study arms may differ not as a result of actual behavioral changes but as a result of different social norms and attitudes. By contrast, if randomization takes places at the unit in which the behavior is observed, then we need not rely on post-experimental surveys but on publicly available precinct-level or municipality-level electoral results.

3.3.4 Pre-randomization Adjustments

The advantages of using cluster randomization come at the cost of statistical efficiency. Under a cluster randomization design, the number of units to be randomized need to be quite large to achieve adequate statistical power. Moreover, the cost per cluster unit of treatment might be large if an excessive number of households belong to the same cluster. I implement two strategies to compensate for the loss of statistical efficiency. First, I truncate my sample of villages to reduce the number of households within units, which increases the cost-efficiency of the per-element treatment delivery. Second, I implement a matching algorithm to oversample villages that were directly exposed to state violence and create a sample of villages that are similar across important pre-treatment covariates. I discuss each strategy in turn.

Sample Truncation

The population of this study are all municipalities of Catalonia with 2,000 eligible voters in the regional elections of 2015, or smaller. The main objective of truncating the sample to small villages is to maximize the efficiency of the intervention. As the unit of randomization and analysis is the village, the delivery of an intervention to a large village would mean to treat a large number of households simply to obtain a single observation. The inclusion of

large villages in the sample is cost-inefficient. Notwithstanding the sample truncation, of all 947 villages in the Catalan region, about 70% of them belong to the population study — 658 villages had less than 2,000 eligible voters.

Pre-randomization Adjustments

An additional approach to reduce the costs of sampling clusters is to capitalize on prior information to increase statistical power. It is well-known that the number of experimental units required to achieve a given level of power is dramatically smaller when units have similar outcomes. Compared to a completely randomized design, blocking experimental units using relevant pre-treatment information can provide significant gains in statistical efficiency (Imai et al., 2009; Raudenbush, Martinez and Spybrook, 2007).

For this purpose, I collect observational data on the electoral results in the previous regional elections of 2012 and 2015 at the level of villages from the regional electoral institute. In addition, I combine them with some demographic baseline characteristics from census data originally reported at the village level by the *Institut d'Estadística de Catalunya* (Idescat). Building on Barceló (2018), I also collect information about the municipalities in which the Spanish police were deployed during the illegal self-determination referendum on October 1.

Because the *collective threat treatment* is based on a reminder of Spanish police forceful action against Catalan separatists, I need to ensure that such an episode of group aggression that triggers collective threat could have realistically taken place in the village. Therefore, municipalities affected by state violence were matched with unaffected municipalities prior to randomization based on their baseline support for pro-independence parties in 2015, turnout rate in 2015, driving distance to the province capital, the log of the number of eligible voters

in 2015, and the province.[95] The objective is to create a sample of 140 villages that could have realistically been the target of state violence in that episode according to a number of pre-treatment covariates.[96]

Complete randomization takes place within 140 villages with the following expectation: half of them are assigned to the treatment group, one quarter to the placebo group, and one quarter to the control group. The distribution of assignment to experimental conditions has two objectives: a roughly even balance of units between treated and non-treated units, and an estimation of the placebo letter effects. Figure 3.4 shows the geographic distribution of the municipalities assigned to each study arm. In total, the distribution 37,000 flyers across 98 villages: 63 villages were assigned to receive treatment letters, 35 were assigned to receive placebo letters, and 42 comparable villages were assigned to receive nothing.

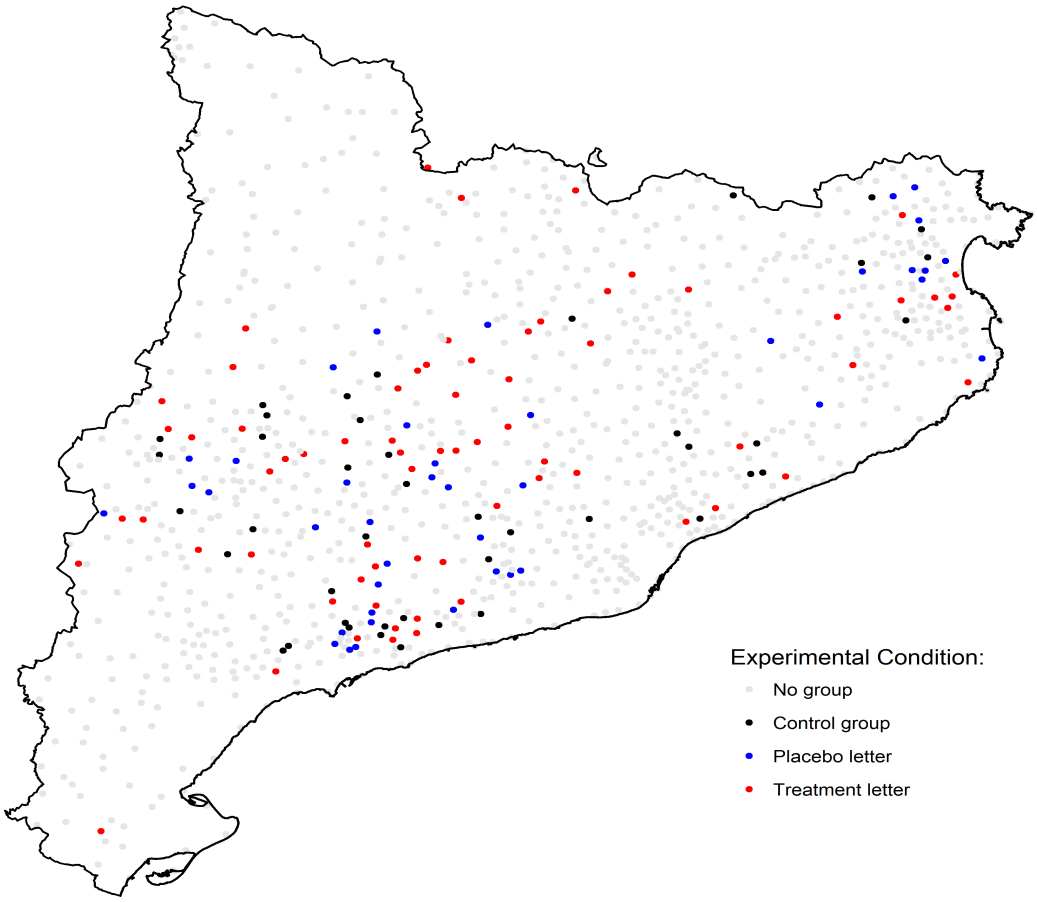
3.3.5 Randomization Checks

A first step in the validation of the research design is to ascertain whether random assignment was conducted correctly. There should be little to no differences between treatment and control groups. Table 3.1 shows the balance in the baseline characteristics of our experimental voting municipalities. The last column of the table presents the p-values of the F-test of joint significance for each independent variable, from a regression of each baseline characteristics

[95] These covariates constitute the main predictors of the geographic distribution of state violence (see Barceló (2018) for further details on what predicted the geographic distribution of the Spanish police deployment). All covariates are matched based on the nearest neighbor matching algorithm. Exact matching is used for the variable of *Province*. Unlike coarsened exact matching, which requires weighing matched observations, nearest matching algorithm uses complete observations.

[96] I ascertain the impact that this decision has on the generalizability of the treatment effects in the online Appendix C.2. The results show that the sample of 140 villages selected here is significantly different from those not selected for the experiment in the following observable factors: (a) exposure to actual violence (by design), and (b) turnout rate (the sample includes villages that turned out 1.07 percentage points less than villages outside the sample of field experiment). The other covariates — namely, vote share for pro-independence parties in 2015, number of eligible voters in 2015, population density, share of Catalan-born population, and province — have no significant differences between the sample and the population of villages.

Figure 3.4: The Geographical Distribution of Experimental Conditions



Note: The use of dots to depict the geographic location of the experimental conditions reflects that leaflets could only be delivered within the urban nucleus of the villages.

on treatment dummies. Similarly, the last row of the table presents the p-values of the F-tests of joint significance of all independent variables, from a regression of each treatment variable on baseline characteristics.

There are no statistically significant differences between the treatment, placebo, and control group village-level voting rates in the past regional elections with regards to voting for pro-independence parties, voting for neutral parties, or turnout rate. Similarly, villages in each group had a similar number of eligible voters in the 2015 regional elections, have a similar population size, are similarly distant to a province capital, and have a similar share of Catalan-born population at the beginning of 2017. The units in each experimental condition are not significantly different in any of these relevant characteristics and, indeed, a joint test of significance suggests that there is not a significant deviation from random assignment.

3.3.6 Quantities of Interest

The central goal of this project is to make causal claims of the impact of collective threat messages on voting behavior, as well as establishing how the effect of priming threat changes as a function of local-level experiences with historical wartime conflict and state repression.

Like many field experiments, the assignment of treatment was randomized, not its reception. The distinction between assignment and reception is critical because not everyone assigned to receive the treatment actually received it. In the context of this field experiment, there are two relevant types of individuals: *compliers* and *never-takers*. *Compliers* are those individuals who receive the treatment leaflet when they reside in a village that is assigned to the treatment group and who do not receive the treatment leaflet when they are assigned to either the placebo or control groups of villages. By contrast, *never-takers* are those individuals

Table 3.1: Baseline Statistics and Orthogonality Tests

	Means and Standard Deviations			P-value from orthogonality test
	Treatment: Threat	Placebo: Non-Threat	Control: No contact	
Electoral outcomes in previous elections:				
Vote for pro-Independence Parties 2015	72.7 (11.6)	71.0 (11.1)	72.4 (12.1)	0.78
Neutral Parties 2015	9.1 (3.3)	8.7 (3.1)	8.8 (3.7)	0.80
Turnout 2015	82.1 (5.0)	81.3 (4.8)	81.3 (4.5)	0.67
Number of Eligible Voters 2015	583.1 (452.3)	695.1 (576.3)	632.5 (527.6)	0.58
Other variables:				
Distance to Province Capital (in km)	94,371.5 (71,012)	97,611.7 (64,707)	99,637.4 (75,850)	0.93
Population size 2017	783.0 (642.4)	946.7 (797.0)	842.3 (727.8)	0.40
Share of Catalan-born Population 2017	82.1 (8.1)	80.0 (8.0)	81.8 (6.9)	0.55
P-value from orthogonality tests	0.85	0.96		

Notes: This table reports baseline summary statistics using publicly available municipality level data. Column (1) presents the means (and standard deviations in parentheses) for the treatment group. Columns (2) and (3) report summary statistics for the placebo and control groups, respectively. The last column present the p-values of a F-test on joint significance of all treatment variables, from a regression of each baseline characteristic on treatment assignment dummies. The last row shows the p-values of a F-test on joint significance of all independent variables, from regressions of each treatment dummy on all baseline covariates.

who never receive the leaflet regardless of whether their village is assigned to the treatment, placebo, or control group.[97]

There are two major sources of never-takers. The first source of noncompliance is binary. The delivery company could not reach some of the villages that were assigned to receive treatment letters.[98] Therefore, all households in these villages are never-takers. A second source of noncompliance is partial. In agreement with the delivery company, only households within the urban nucleus of villages would receive leaflets. In other words, isolated houses, households in industrial parks or in residential areas not attached to a urban center were excluded from the delivery areas. Hence, the proportion of households outside the urban nucleus of a village provides a lower bound of the number of never-takers. Fortunately, we have precise information about this lower bound from the *Institut d'Estadística de Catalunya* (Idescat). On average, in our sample of villages, 66.3% of the individuals who live in our sample of villages reside within the delivery areas.

None of the two types of compliance are random. Therefore, I begin my analyses by estimating an intent-to-treat effect; that is, the effect of assigned (rather than actual) treatment on my outcome of interest. This is the most conservative estimate as it ignores noncompliance. The following regression framework is used to estimate the treatment effects:

$$Vote\ Indep_{i2017} = \alpha\ + \beta\ Treat\ Assign_i + \delta\ Vote\ Indep_{i2015} + \epsilon_i,$$

[97]The other two compliance statuses that are irrelevant in the field experiment are *always-takers* and *defiers*. *Always-takers* are those who always receive the treatment leaflet regardless of whether their village is assigned to the treatment, placebo, or control group. We can reasonably expect that these are very few people, if anybody at all, because of the design of the experiment. *Defiers* are those who receive the treatment leaflet when they live in a village assigned to receive the placebo leaflets or nothing and who receive a placebo or nothing when they live in a village assigned to receive the treatment leaflet. We can assume that there are no defiers.

[98]Of the 98 villages that were assigned to receive leaflets, letters could not be delivered to four villages assigned to treatment letter: La Galera, Montclar, Santa Maria de Merlès i Santa Maria de Besora. The delivery company had two days to deliver the material to households. After the intervention, they informed us that they were unable to reach these four villages due to time constraints.

However, our primary quantity of interest has to do with the efficacy of the actual treatment mechanism. For this, I recover a complier average causal effect (CACE) to gauge the effect of the treatment under non-compliance circumstances, as first suggested by Angrist, Imbens and Rubin (1996).[99] Hence, I regress the percentage of votes for pro-independence parties in 2017 on the proportion of the households that could receive a letter in the village and the percentage of votes for pro-independence parties in 2015, and instrument the treatment with the assignment to the treatment group.

$$\pi_i Treat\ Letter_i = \alpha_0\ + \alpha_1\ Treat\ Assign_i + \delta\ Vote\ Indep_{i2015} + \epsilon_i,$$

$$Vote\ Indep_{i2017} = \beta_0\ +\ \beta_1\ \pi_i \widehat{Treat\ Letter}_i + \beta_2\ Vote\ Indep_{i2015} + \epsilon_i,$$

To test the effect on polarization, I substitute the voteshare for pro-independence parties for the voteshare for neutral and unionist parties, respectively. Further, when I test the conditional effect of the treatment condition on historical wartime violence, I regress the voteshare for pro-independence parties in 2017 on the interaction between assignment to the treatment letter and the measure of historical wartime violence and the voteshare for pro-independence parties in 2015.

[99]The assumptions underlying CACE, namely monotonicity, stable unit treatment value, random assignment, and the exclusion restriction, are naturally met given the research design. In brief, defiers are implausible, interference across units is unlikely due to the aggregate nature of the level of randomization and the scattered geographic distribution of sampled villages, the random assignment of the treatment, and the lack of alternative channels between assignment and outcome other than the actual treatment. However, the interpretation of the CACE assumes homogeneity; that is, the treatment effect would have been similar among those households outside the urban nucleus than those within the urban nucleus.

3.4 Results

I divided the results into three subsections. First, I discuss the effects of priming collective threat on voting for pro-Catalan, secessionists, parties. Second, I examine the effects the treatment had on party polarization. Finally, I test the conditional effects of historical experiences on voting behavior.

3.4.1 The Effect of Collective Threat Messages on Vote Choice

Table 3.2 presents the effects of the primes of collective threat on villages' voting behavior. The dependent variable in columns 1 through 4 is the percentage of votes in favor of pro-independence parties in 2017. As all models include the lagged dependent variable as a covariate, coefficients report the intent-to-treat effects as expected shifts in percentage points in the outcome of interest between 2015 and 2017. Columns (1) and (3) show that placebo letters had no significant influence on voting behavior. All coefficients are virtually zero. Hence, even columns collapse the placebo and the control groups. This decision substantially increases the statistical power of the test in the estimation of the collective threat experimental treatment and, thus, I discuss the results from these specifications.

The main finding is that the leaflet campaign with collective threat messages leads to a 0.74 percentage points (p-value = 0.04) increase in support for Catalan pro-independence parties. Given that support for pro-independence parties in the placebo and control groups is 71-72.4%, the overall increase in voting for pro-independence parties amounts to 1.02-1.04%. These estimates are overly conservative as they do not account for the practical issues in the field experiment that I alluded to before.

Table 3.2: Estimates of the Effect of Collective Threat Messages on Voting Behavior

	DV: % Pro-Independence, 2017			
	ITT		*CACE*	
	(1)	(2)	(3)	(4)
Threat letter	0.74*	0.74**	1.23*	1.22**
	(0.49)	(0.42)	(0.83)	(0.71)
Placebo letter	−0.00		0.01	
	(0.57)		(0.88)	
Lagged DV: % Vote Pro-Independence, 2015	0.97***	0.97***	0.97***	0.97***
	(0.02)	(0.02)	(0.02)	(0.02)
N Villages	140	140	140	140

Note: Constant omitted from the regression output. *p<0.1; **p<0.05; ***p<0.01. One-tailed tests.

Columns (3) and (4) report the instrumental variable estimation models to approximate the actual efficacy of exposure to the leaflet. After accounting for noncompliers in the implementation of the field experiment, the leaflet campaign with primes of collective threat leads to an increase of 1.23 percentage points (p-value = 0.04) in support for pro-independence parties among those villages that were actually exposed to the collective threat treatment. Given the average support for pro-independence parties in the placebo and control groups, the overall increase in voting for pro-independence parties amounts to 1.73%. For this estimate to be a true *CACE*, we should assume that everyone who lives in the urban nucleus of treated villages have been exposed to the treatment leaflets. Departures from this assumption would bias downward the CACE estimates.

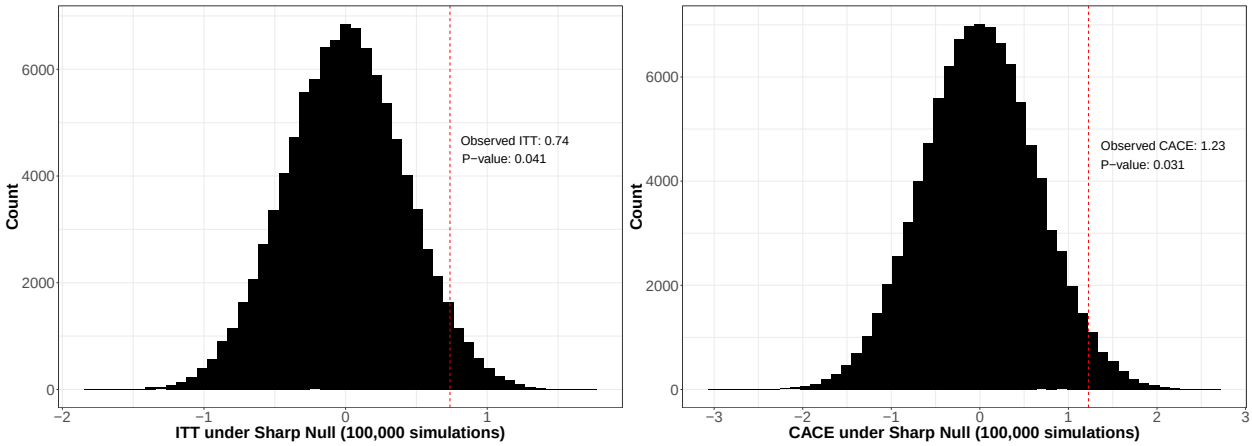
To assess how likely these differences in support for pro-independence parties would have arisen by chance, I proceed with the randomization inference framework to test their sharp null hypotheses. The sharp null calculates p-values under the hypothesis of no treatment

effect for all observations. Hence, I examine whether differences in variation in support for pro-independence parties between treated and control villages as large as the observed differences would have been observed if the primes of collective threat did not influence electoral outcomes. Assuming this sharp null hypothesis, I compute a complete schedule of potential outcomes, from which I generate a randomization distribution by randomly sampling 100,000 possible vectors of treatment assignments.

Figure C.3 shows the distribution of the simulated ITT and CACE effects under random assignment of the treatment in these 100,000 possible assignments. The left panel shows the distribution of 100,000 simulations of the ITT under a strict null of no effect. The red and dashed line indicate the observed ITT effect of the experiment. The p-value indicates the percentage of simulated effects under random assignment that are greater than the observed treatment effect. Overall, we observe that only 4.1% of the simulated ITT effects are as large as the observed differences.

The right panel shows the distribution of 100,000 simulations of the ITT under a strict null of no effect. The estimated CACE is obtained from implementing the instrumental variable method discussed above for each randomly generated treatment vector. The red and dashed line indicate the observed CACE of the experiment. Again, the p-value indicates the percentage of simulated effects under random assignment that are greater than the observed treatment effect. Overall, we observe that only 3.1% of the simulated ITT effects are as large as the observed differences. In both cases, the test of the sharp null suggest that the observed effects of the treatment of the field experiment are unlikely to be due to chance alone.

Figure 3.5: Randomization Distribution of Effects Under a Strict Null



3.4.2 The Effect of Collective Threat Messages on Polarization

The positive effect of the leaflet campaign leads to the question of whether primes of collective threat increase party polarization. On the one hand, a party system would become more polarized if the rise of pro-independence parties in treated villages is at the expense of neutral or central parties. On the other hand, a party system would not become more polarized if pro-independence parties rise at the expense of unionist parties. In the latter case, the entire political system would shift toward more pro-independence parties but the distances among parties would remain the same.

Table 3.3 summarizes the effects of collective threat on vote choice in favor of neutral and unionist parties. The leaflet campaign leads to between 0.54 and 0.65 percentage points decrease (p-value = 0.05) in support for neutral parties, those that are neither secessionist nor unionist. Given that the support for neutral parties is 8.7% in the placebo and control villages, the overall decrease in voting for neutral parties ranges between 6.3% and 7.5% depending on the model specification. When I account for the lower bound of compliance

in the implementation of the field experiment, the decrease in percentage points ranges between 0.90 and 1.07. This is equivalent to a decrease between 10.5% and 12.5% in the vote percentage for neutral parties. Based on the randomization inference framework, we can observe that more than 90% of the random vectors of treatment assignment yield an ITT effect that is lower than the observed differences and more than 95% of the random vectors of treatment assignment yield an CACE effect that is lower than the observed differences. These results suggest that these differences across villages of treatment conditions in support for neutral parties are unlikely to have arisen by chance alone.[100]

By contrast, the campaign of primes of collective threat has little effect on vote support for unionist parties. Specifically, a village's assignment to treatment letters leads to an increase from 0.06 to 0.13 percentage points in support for unionist parties (p-value > 0.50). This increase is not statistically distinguishable from a null result. Given the average support for unionist parties in the placebo and control groups, this is equivalent to an increase from 0.3% to 0.6% in support for unionist parties. Accounting for the upper bound of compliance slightly increases the magnitude of the effects: the assignment to receive treatment letters in a village leads to an increase from 0.07 to 0.22 percentage points (p-value > 0.50) in support for unionist parties, which is equivalent to an increase from 0.4% to 1%. The effect is unreliable and far from standard thresholds of statistical significance. Similarly, the test of the sharp null hypothesis suggests that these differences in support for unionist parties might have easily arisen by chance.

Overall, the results indicate that the increased vote share of Catalan pro-independence parties happens to the detriment of neutral parties. This evidence provides support for hypothesis 2 by which priming collective threat creates a more polarized political landscape because

[100]The online Appendix C.6 provide further details on the tests against the sharp null hypothesis for the effect of the treatment on the support for neutral and unionist parties, respectively.

Table 3.3: Estimates of the Effect of Collective Threat Messages on Polarization

	DV: % Vote for Neutral Parties, 2017			
	(1.1)	(1.2)	(1.3)	(1.4)
Threat letter	-0.64^{*}	-0.54^{*}	-1.05^{*}	-0.90^{*}
	(0.39)	(0.33)	(0.66)	(0.56)
Placebo letter	-0.21		-0.33	
	(0.45)		(0.70)	
Omitted Group	No letter	Placebo or no letter	No letter	Placebo or no letter
Estimator	ITT	ITT	CACE	CACE
	DV: % Vote for Unionist Parties, 2017			
	(2.1)	(2.2)	(2.3)	(2.4)
Threat letter	0.12	0.06	0.14	0.07
	(0.53)	(0.45)	(0.87)	(0.74)
Placebo letter	0.14		0.14	
	(0.61)		(0.93)	
Estimator	ITT	ITT	CACE	CACE

Note: *p<0.1; **p<0.05; ***p<0.01. One-tailed tests. Model specifications are the same as those in Table 3.2. All columns include the lagged dependent variable as measured from the electoral results in 2015. The online Appendix C.5 shows the full table of results.

treated communities reduce their support for neutral parties in favor of pro-independence parties.

3.4.3 The Conditional Effects of Historical Experiences

In Table 3.4, I examine the degree to which local-level historical violence conditions the effects of collective threat messages on the support for pro-independence political parties.[101] Table 3.4 shows that the interaction term between the treatment letter and historical violence is consistently positive and significant when I estimate both the ITT (columns 1 and 2) and the CACE (columns 3 and 4). By contrast, the placebo letter has no effect on the increased support for pro-independence parties at any level of local historical experience in any of the models.

Figure 3.6 presents an illustration of these results. The left and right panels show the marginal effects of the intent-to-treat effects and the complier average causal effect, respectively, along the range of observed values in the variable *historical experiences*. The upper panels display the marginal plots of both the treatment and the placebo. Both plots show that the placebo letters had no effect at any value of historical experiences. Therefore, I will focus my discussion on the bottom panels as they combine the placebo and the control groups to strengthen statistical power.

The left panels show significant heterogeneous effects of the treatment across the range of observed values of historical experiences. The treatment letter had a small, and not significant, impact in those municipalities that experienced minimal historical wartime violence. As the level of direct exposure to such historical events increases, priming contemporary collective threat has substantially stronger effects on contemporary political behavior.

[101] I check the balance of the moderator across the experimental conditions in the online Appendix C.7. In brief, four two-sample Kolmogorov-Smirnov tests (treatment vs. placebo, treatment vs. control, treatment vs. placebo and control, and placebo vs. control) show no significant differences in the historical violence experiences across experimental conditions.

Table 3.4: The Effects of Priming Collective Threat Conditional on Historical Experiences

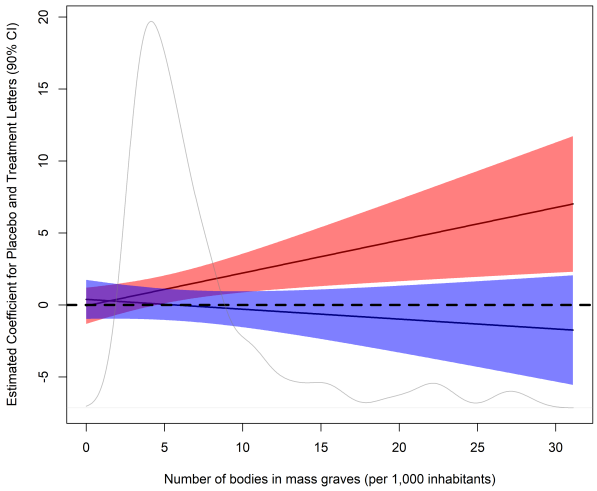
	DV: % Vote Pro-Independence, 2017			
	ITT		*CACE*	
	(1)	(2)	(3)	(4)
Threat Letter	−0.09	−0.25	−0.11	−0.37
	(0.64)	(0.54)	(1.08)	(0.92)
Placebo Letter	0.34		0.50	
	(0.69)		(1.09)	
Historical Violence	0.02	−0.005	0.02	−0.01
	(0.05)	(0.04)	(0.05)	(0.04)
Threat Letter	0.23***	0.26***	0.38**	0.42***
× Historical Violence	(0.09)	(0.08)	(0.15)	(0.14)
Placebo Letter	−0.06		−0.08	
× Historical Violence	(0.07)		(0.10)	
Lagged DV: % Vote	0.98***	0.98***	0.98***	0.98***
Pro-Independence, 2015	(0.02)	(0.02)	(0.02)	(0.02)
N Villages	140	140	140	140

Note: Constant omitted from the regression output. *p<0.1; **p<0.05; ***p<0.01. One-tailed tests.

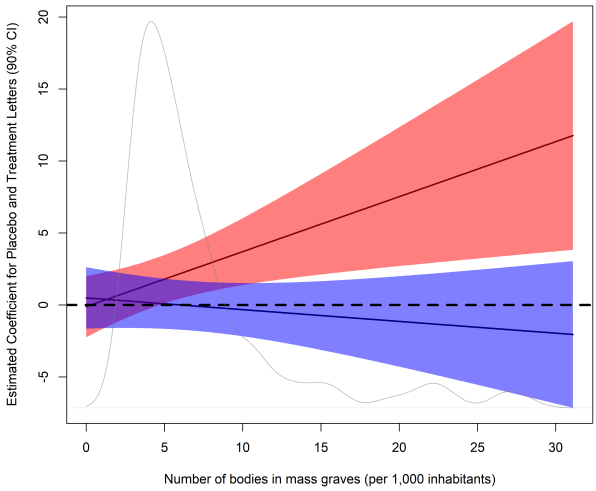
The marginal effects of the ITT and CACE models show that for a town with an average exposure to historical violence — 5 bodies per thousand inhabitants in mass graves — the support for pro-independence parties is expected to increase by 1.04 percentage points among units assigned to the treatment and 1.72 percentage points among compliers, which is equivalent to an increase of 1.43% and 2.37%, respectively. When we take a town with 1 standard deviation above the mean in the historical experience with wartime violence or state repression — 11 bodies per thousand inhabitants in mass graves — priming contemporary collective threat bolsters the expected increase in the support for pro-independence parties by 2.58 percentage points among those assigned to the treatment and 4.24 percentage points among the treatment compliers, which is equivalent to an increase of 3.56% and 5.84%, respectively. By contrast, priming contemporary violence exerts no significant effect on political behavior for a town with 1 standard deviation below the mean in the moderator regardless of whether we use the ITT or the CACE estimator.

Overall, we see that within one standard deviation around the mean, the effects of the treatment range from zero to 3.56% among units assigned to the treatment and from 0 to 5.84% among complier units, which suggests substantial heterogeneous effects of treatment conditional on the historical local-level experiences with wartime violence. This evidence supports hypothesis 3.

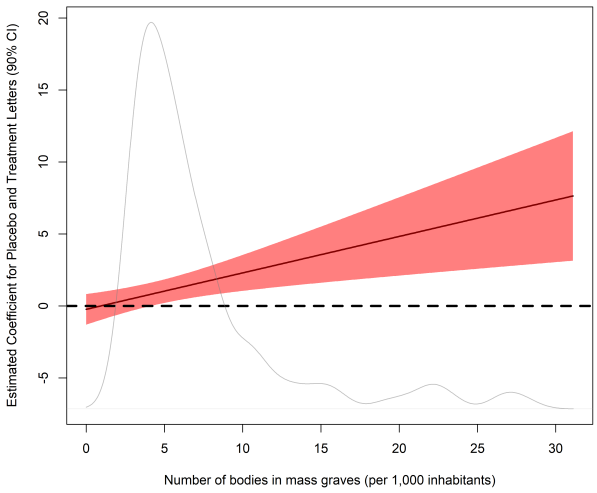
Figure 3.6: The ITT and CACE of Priming Collective Threat Conditional on Historical Experiences



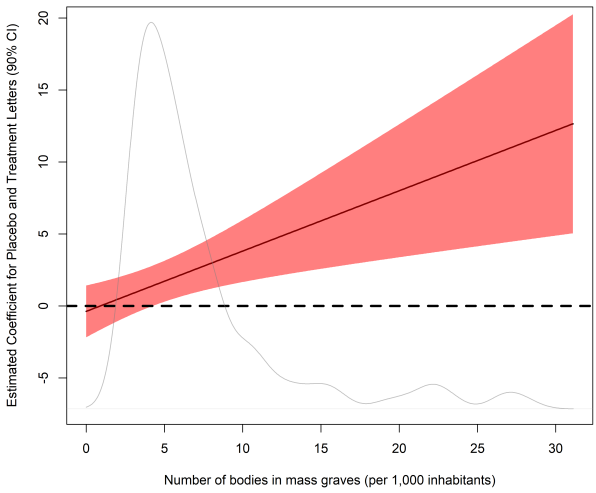
Treatment and Placebo Effects (ITT)



Treatment and Placebo Effects (CACE)



Treatment Effects (ITT)



Treatment Effects (CACE)

Note: The density distribution of the number of bodies per capita buried in municipalities is plotted in the background of the figures.

3.5 Conclusion

Do messages of collective threat influence voting behavior? This paper reports the results of a region-wide field experiment conducted during a pro-independence leaflet campaign in the 2017 Catalan regional election in Spain. The local NGO designed the campaign to induce citizens with a sense of collective threat—by reminding voters about a recent episode of police brutality by Spanish police forces—which was expected to favor nationalist-secessionist parties. The experimental intervention spanned all four Catalan provinces, encompassed 140 Catalan ethno-linguistic communities, and reached an estimated 120,000 inhabitants (including control villages). The findings show that in villages assigned to receive a collective threat prime, the vote share of Catalan pro-secessionist political parties increased by between 0.74 and 1.22 percentage points–which is equivalent to 1% and 1.7% increase–compared to zero-increase of precincts that received a vote encouragement with either no collective threat leaflet or no leaflet at all.

Which groups of people are more likely to be influenced by such messages? Added to the main effects of the collective threat messages, I have suggested a resonance model of political behavior to help explain which groups of people are more likely to be swayed by such messages. I draw evidence from the size and location of more than 2,650 mass graves scattered throughout Spain to test whether people who live in villages that were more severely affected by the Spanish Civil War and the ensuing Francoist repression (1936–1945) are more likely to be mobilized by contemporary messages of collective threat. I find that the historical political context strongly conditions the impact of threat messages. More specifically, the impact of collective threat messages leads to a significant increase of between 3.6% and 5.8% in the support for Catalan pro-secessionist parties among those communities that were more heavily affected by the Spanish Civil War and the post-war repression. This contrasts to the

significant, yet more moderate, increase of between 1.4% and 2.4% in those villages with an average history of wartime violence and state repression.

This study contributes to a large literature on the drivers and effects of persuasive communication in divided societies, as well as the emerging literature on the historical legacies of conflict processes, and extend them in several important directions. Scholars have reported mixed findings about the role of leaders in shaping voting behavior (Horowitz and Klaus, 2018; Mutz, 2018; Selb and Munzert, 2018). This paper shows the importance of leaders' threat messages in mobilizing people in favor of ethno-nationalist parties on the basis of a field experiment with actual electoral outcomes.

The causal impact of fear-based appeals on the support for nationalist parties suggests a plausible mechanism for their rise around the world. Their success may not simply be the result of certain economic context (Colantone and Stanig, 2018), but they may also reflect the potency of fear-based appeals highlighting the potential of an external threat. However, further research is required to establish whether fear-based appeals are only (or more likely to be) effective at boosting support for a particular set of parties (e.g., nationalist, populist, right-wing) or, by contrast, these messages might work equally well across the entire political spectrum.

The conditional impact of threat appeals has implications on how historical events leave long-lasting legacies on contemporary behavior. On the one hand, the finding that threat-based appeals are more effective at shaping political behavior in those areas that have been exposed to historical conflict provides a plausible explanation on the geographic clustering of conflict locations (Braithwaite, 2005; Buhaug and Gleditsch, 2008). In other words, the recurrence of conflict in similar geographic units could arguably be the result of the greater susceptibility

to fear-based appeals of those who had been socialized in a context of a significant record of political violence—whose place of residence tend to be postconflict areas.

While this paper empirically focuses on the relationship between collective threat appeals, violence, and political behavior, these findings have general implications on the literature of public opinion. More generally, the results reveal that contextual conditions may magnify the effect of political rhetoric on actual political behavior. In the future, scholars may want to consider whether collective threat rhetoric (e.g., immigrants) might be more effective in geographic areas with particular contextual conditions (e.g., exposure to globalization).

References

Abramson, Paul R. 1983. *Political attitudes in America: Formation and change*. San Francisco, CA: W.H.Freeman Co Ltd.

Acharya, Avidit, Matthew Blackwell and Maya Sen. 2016. "The political legacy of American slavery." *The Journal of Politics* 78(3):621–641.

Adolph, Christopher. 2013. *Bankers, Bureaucrats, and Central Bank Politics: The Myth of Neutrality*. Cambridge, MA: Cambridge University Press.

Allport, Gordon W. 1954. *The nature of prejudice*. Cambridge, MA: Addison-Wesley.

Almond, Gabriel Abraham and Sidney Verba. 1963. *The civic culture: Political attitudes and democracy in five nations*. Princeton, NJ: Princeton University Press.

Angrist, Joshua D, Guido W Imbens and Donald B Rubin. 1996. "Identification of causal effects using instrumental variables." *Journal of the American statistical Association* 91(434):444–455.

Ansolabehere, Stephen D, Shanto Iyengar and Adam Simon. 1999. "Replicating experiments using aggregate and survey data: The case of negative advertising and turnout." *American Political Science Review* 93(4):901–909.

Arceneaux, Kevin. 2007. "I'm asking for your support: The effects of personally delivered campaign messages on voting decisions and opinion formation." *Quarterly Journal of Political Science* 2:43–65.

Arceneaux, Kevin. 2010. "The benefits of experimental methods for the study of campaign effects." *Political Communication* 27(2):199–215.

Arceneaux, Kevin and David W Nickerson. 2009. "Who is mobilized to vote? A re-analysis of 11 field experiments." *American Journal of Political Science* 53(1):1–16.

Atkinson, Carol. 2010. "Does soft power matter? A comparative analysis of student exchange programs 1980–2006." *Foreign Policy Analysis* 6(1):1–22.

Bailey, Michael A, Daniel J Hopkins and Todd Rogers. 2016. "Unresponsive and unpersuaded: The unintended consequences of a voter persuasion effort." *Political Behavior* 38(3):713–746.

Balcells, Laia. 2012. "The consequences of victimization on political identities: Evidence from Spain." *Politics & Society* 40(3):311–347.

Balcells, Laia. 2017. *Rivalry and revenge*. Cambridge, MA: Cambridge University Press.

Barabas, Jason and Jennifer Jerit. 2010. "Are survey experiments externally valid?" *American Political Science Review* 104(2):226–242.

Barceló, Joan. 2014. "Contextual effects on subjective national identity." *Nations and Nationalism* 20(4):701–720.

Barceló, Joan. 2018. "Batons and ballots: The effectiveness of state violence in fighting against Catalan separatism." *Research & Politics* 5(2):1–9.

Barceló, Joan and Taishi Muraoka. 2019. "The ideological legacy of regime transitions: Political conservatism as a response to political uncertainty." *Working Paper* .

Barton, Jared, Marco Castillo and Ragan Petrie. 2014. "What persuades voters? A field experiment on political campaigning." *The Economic Journal* 124(574):F293–F326.

Bauer, Michal, Christopher Blattman, Julie Chytilová, Joseph Henrich, Edward Miguel and Tamar Mitts. 2016. "Can war foster cooperation?" *Journal of Economic Perspectives* 30(3):249–274.

Baum, Matthew A. 2002. "Sex, lies, and war: How soft news brings foreign policy to the inattentive public." *American Political Science Review* 96(1):91–109.

Bausch, Andrew W. 2015. "Democracy, War Effort, and the Systemic Democratic Peace." *Journal of Peace Research* 52(4):435–447.

Beber, Bernd, Philip Roessler and Alexandra Scacco. 2014. "Intergroup violence and political attitudes: Evidence from a dividing Sudan." *The Journal of Politics* 76(3):649–665.

Bellows, John and Edward Miguel. 2006. "War and institutions: New evidence from Sierra Leone." *The American Economic Review* 96(2):394–399.

Bellows, John and Edward Miguel. 2009. "War and local collective action in Sierra Leone." *Journal of Public Economics* 93(11):1144–1157.

Berelson, Bernard, Paul F Lazarsfeld and William McPhee. 1954. *Voting: A Study of Opinion Formation in a Presidential Campaign*. Chicago, IL: University of Chicago Press.

Berkman, Michael B and Robert E O'Connor. 1993. "Do women legislators matter? Female legislators and state abortion policy." *American Politics Quarterly* 21(1):102–124.

Berrebi, Claude and Esteban F Klor. 2008. "Are voters sensitive to terrorism? Direct evidence from the Israeli electorate." *American Political Science Review* 102(3):279–301.

Besley, Timothy, Jose G Montalvo and Marta Reynal-Querol. 2011. "Do educated leaders matter?" *The Economic Journal* 121(554):F205–227.

Besley, Timothy and Marta Reynal-Querol. 2011. "Do democracies select more educated leaders?" *American Political Science Review* 105(03):552–566.

Besley, Timothy and Marta Reynal-Querol. 2014. "The legacy of historical conflict: Evidence from Africa." *American Political Science Review* 108(2):319–336.

Blattman, Christopher. 2009. "From violence to voting: War and political participation in Uganda." *American Political Science Review* 103(2):231–247.

Blattman, Christopher and Jeannie Annan. 2010. "The consequences of child soldiering." *The Review of Economics and Statistics* 92(4):882–898.

Boehm-Tabib, Eti. 2016. "Acute stress disorder among civilians during a war and post-traumatic growth six years later: The impact of personal and social resources." *Anxiety, Stress, & Coping* 29(3):318–333.

Bonanno, George A, Sandro Galea, Angela Bucciarelli and David Vlahov. 2006. "Psychological resilience after disaster New York city in the aftermath of the September 11th Terrorist Attack." *Psychological Science* 17(3):181–186.

Bond, Robert M, Christopher J Fariss, Jason J Jones, Adam DI Kramer, Cameron Marlow, Jaime E Settle and James H Fowler. 2012. "A 61-million-person experiment in social influence and political mobilization." *Nature* 489(7415):295–298.

Brader, Ted. 2005. "Striking a responsive chord: How political ads motivate and persuade voters by appealing to emotions." *American Journal of Political Science* 49(2):388–405.

Brader, Ted. 2006. *Campaigning for hearts and minds: How emotional appeals in political ads work*. Chicago, IL: University of Chicago Press.

Brader, Ted, Nicholas Valentino and Elizabeth Suhay. 2008. "Is it immigration or the immigrants? The emotional influence of groups on public opinion and political action." *American Journal of Political Science* 52(4):959–978.

Brady, Henry E and Richard Johnston. 2006. *Capturing campaign effects*. Ann Arbor, MI: University of Michigan Press.

Braithwaite, Alex. 2005. "Location, location, location... identifying hot spots of international conflict." *International Interactions* 31(3):251–273.

Brück, Tilman, Patricia Justino, Philip Verwimp, Alexandra Avdeenko and Andrew Tedesco. 2016. "Measuring violent conflict in micro-level surveys: current practices and methodological challenges." *The World Bank Research Observer* 31(1):29–58.

Buhaug, Halvard and Kristian Skrede Gleditsch. 2008. "Contagion or confusion? Why conflicts cluster in space." *International Studies Quarterly* 52(2):215–233.

Calhoun, Lawrence G and Richard G Tedeschi. 1999. *Facilitating posttraumatic growth: A clinician's guide*. Mahwah, NJ: Lawrence Erlbaum Associates Inc.

Campbell, Angus, Philip E Converse, Warren E Miller and Donald E Stokes. 1960. *The American voter*. Chicago, IL: University of Chicago Press.

Campbell, James E. 2010. "Explaining politics, not polls: Reexamining macropartisanship with recalibrated NES Data." *Public Opinion Quarterly* 74(4):616–642.

Cardy, Emily Arthur. 2005. "An experimental field study of the GOTV and persuasion effects of partisan direct mail and phone calls." *The Annals of the American Academy of Political and Social Science* 601(1):28–40.

Carlyle, Thomas. 1841. "The hero as divinity. Odin'." *On Heroes, Hero-Worship and the Heroic in History (1841; New York: John Wiley, 1866)* p. 31.

Carnes, Nicholas. 2012. "Does the Numerical Underrepresentation of the Working Class in Congress Matter?" *Legislative Studies Quarterly* 37(1):5–34.

Carnes, Nicholas and Noam Lupu. 2015. "Rethinking the Comparative Perspective on Class and Representation: Evidence from Latin America." *American Journal of Political Science* 59(1):1–18.

Carnes, Nicholas and Noam Lupu. 2016. "What good is a college degree? Education and leader quality reconsidered." *The Journal of Politics* 78(1):35–49.

Carter, David B and Curtis S Signorino. 2010. "Back to the future: Modeling time dependence in binary data." *Political Analysis* 18(3):271–292.

Cassar, Alessandra, Pauline Grosjean and Sam Whitt. 2013. "Legacies of violence: Trust and market development." *Journal of Economic Growth* 18(3):285–318.

CatSalut. 2017. Informe sobre els incidents dels dies 1 al 4 d'octubre de 2017 Pacients atesos durant la jornada electoral i dies posteriors a conseqüència de les càrregues dels cossos policials de l'Estat. Barcelona, Spain: Departament de Salut, Generalitat de Catalunya.

Chandra, Kanchan. 2007. *Why ethnic parties succeed: Patronage and ethnic head counts in India*. Cambridge, MA: Cambridge University Press.

Checkel, Jeffrey T. 2005. "International institutions and socialization in Europe: Introduction and framework." *International Organization* pp. 801–826.

Chi, Do Quynh and Di van den Broek. 2013. "Wildcat strikes: A catalyst for union reform in Vietnam?" *Journal of Industrial Relations* 55(5):783–799.

Child, Travers Barclay and Elena Nikolova. 2018. "War and social attitudes." *Conflict Management and Peace Science* pp. 1–20.

Chiozza, Giacomo and Hein Erich Goemans. 2011. *Leaders and international conflict*. Cambridge, MA: Cambridge University Press.

Chong, Dennis and James N Druckman. 2007*a*. "Framing public opinion in competitive democracies." *American Political Science Review* 101(4):637–655.

Chong, Dennis and James N Druckman. 2007*b*. "A theory of framing and opinion formation in competitive elite environments." *Journal of Communication* 57(1):99–118.

Christensen, Henrik Serup. 2015. "Power sharing and political dissatisfaction: A multilevel analysis of the link between power-sharing institutions and kinds of political dissatisfaction in 24 European democracies." *European Politics and Society* 16(2):280–300.

Claassen, Christopher. 2016. "Group entitlement, anger and participation in intergroup violence." *British Journal of Political Science* 46(1):127–148.

Colantone, Italo and Piero Stanig. 2018. "Global competition and Brexit." 112(2):201–218.

Colgan, Jeff D. 2013. "Domestic revolutionary leaders and international conflict." *World Politics* 65(04):656–690.

Colletta, Nat J and Michelle L Cullen. 2000. *Violent conflict and the transformation of social capital: Lessons from Cambodia, Rwanda, Guatemala, and Somalia*. Vol. 795 World Bank Publications.

Collier, Paul and Anke Hoeffler. 2004. "Greed and Grievance in Civil War." *Oxford Economic Papers* 56(4):563–595.

Collier, Paul et al. 2003. *Breaking the conflict trap: Civil war and development policy*. World Bank Publications.

Conley, Timothy G, Christian B Hansen and Peter E Rossi. 2012. "Plausibly exogenous." *Review of Economics and Statistics* 94(1):260–272.

Cramer, Christopher. 2006. *Civil war is not a stupid thing: Accounting for violence in developing countries*. London, UK: Hurst & Company.

Croco, Sarah E and Jessica LP Weeks. 2016. "War Outcomes and Leader Tenure." *World Politics* 68(4):1–31.

Crosby, Alfred W. 1986. *Ecological Imperialism: The Biological Expansion of Europe 900-1900*. New York: Cambridge University Press.

Dafoe, Allan. 2011. "Statistical Critiques of the Democratic Peace: Caveat emptor." *American Journal of Political Science* 55(2):247–262.

Dafoe, Allan and Devin Caughey. 2016. "Honor and War." *World Politics* 68(02):341–381.

Dafoe, Allan, Jonathan Renshon and Paul Huth. 2014. "Reputation and Status as Motives for War." *Annual Review of Political Science* 17:371–393.

Dahlberg, Stefan, Sören Holmberg, Bo Rothstein, Anna Khomenko and Richard Svenson. 2016. "The quality of Government Basic Dataset, version January 2016.".

De Figueiredo, Rui and Barry Weingast. 1999. "The rationality of fear: Political opportunism and ethnic conflict." *Civil wars, insecurity, and intervention* pp. 261–302.

De Juan, Alexander and Jan Henryk Pierskalla. 2016. "Civil war violence and political trust: Microlevel evidence from Nepal." *Conflict Management and Peace Science* 33(1):67–88.

De Luca, Giacomo and Marijke Verpoorten. 2015. "Civil war and political participation: evidence from Uganda." *Economic Development and Cultural Change* 64(1):113–141.

De Mesquita, Bruce Bueno, James D Morrow, Randolph M Siverson and Alastair Smith. 1999. "An Institutional Explanation of the Democratic Peace." *American Political Science Review* 93(04):791–807.

Deininger, Klaus. 2003. "Causes and consequences of civil strife: micro-level evidence from Uganda." *Oxford Economic Papers* 55(4):579–606.

Dell, Melissa and Pablo Querubin. 2018. "Nation building through foreign intervention: Evidence from discontinuities in military strategies." *The Quarterly Journal of Economics* 133(2):701–764.

Dewan, Torun, Macartan Humphreys and Daniel Rubenson. 2014. "The elements of political persuasion: content, charisma and cue." *The Economic Journal* 124(574).

Diamond, Larry. 1994. *Political Culture and Democracy in Developing Countries: Textbook edition*. Lynne Rienner Publishers.

Druckman, James N. 2004. "Priming the vote: Campaign effects in a US Senate election." *Political Psychology* 25(4):577–594.

Druckman, James N, Matthew S Levendusky and Audrey McLain. 2018. "No Need to Watch: How the Effects of Partisan Media Can Spread via Interpersonal Discussions." *American Journal of Political Science* 62(1):99–112.

Dunning, Thad. 2011. "Fighting and voting: Violent conflict and electoral politics." *Journal of Conflict Resolution* 55(3):327–339.

Eifert, Benn, Edward Miguel and Daniel N Posner. 2010. "Political competition and ethnic identification in Africa." *American Journal of Political Science* 54(2):494–510.

Esteban, Joan, Laura Mayoral and Debraj Ray. 2012*a*. "Ethnicity and Conflict: An Empirical Study." *The American Economic Review* pp. 1310–1342.

Esteban, Joan, Laura Mayoral and Debraj Ray. 2012*b*. "Ethnicity and Conflict: Theory and Facts." *Science* 336(6083):858–865.

Fearon, James D. 1994. "Domestic Political Audiences and the Escalation of International Disputes." *American Political Science Review* 88(03):577–592.

Fearon, James D and David D Laitin. 2003. "Ethnicity, Insurgency, and Civil War." *American Political Science Review* 97(01):75–90.

Finkel, Steven E. 1993. "Reexamining the" minimal effects" model in recent presidential campaigns." *The Journal of Politics* 55(1):1–21.

Finkel, Steven E and Howard R Ernst. 2005. "Civic Education in Post-apartheid South Africa: Alternative Paths to the Development of Political Knowledge and Democratic Values." *Political Psychology* 26(3):333–364.

Freyburg, Tina. 2015. "Transgovernmental Networks as an Apprenticeship in Democracy? Socialization into Democratic Governance through Cross-national Activities." *International Studies Quarterly* 59(1):59–72.

Fuhrmann, Matthew and Michael C Horowitz. 2015. "When Leaders Matter: Rebel Experience and Nuclear Proliferation." *The Journal of Politics* 77(1):72–87.

Galovski, Tara and Judith A Lyons. 2004. "Psychological sequelae of combat violence: A review of the impact of PTSD on the veteran's family and possible interventions." *Aggression and Violent Behavior* 9(5):477–501.

Gerber, Alan S and Donald P Green. 2000. "The effects of canvassing, telephone calls, and direct mail on voter turnout: A field experiment." *American political science review* 94(3):653–663.

Gerber, Alan S and Donald P Green. 2017. Field Experiments on Voter Mobilization: An Overview of a Burgeoning Literature. In *Handbook of Economic Field Experiments*. Vol. 1 Elsevier pp. 395–438.

Getmansky, Anna and Thomas Zeitzoff. 2014. "Terrorism and voting: The effect of rocket threat on voting in Israeli elections." *American Political Science Review* 108(3):588–604.

Gift, Thomas and Daniel Krcmaric. 2016. "Who democratizes? Western-educated leaders and regime transitions." *Journal of Conflict Resolution* .

Gilligan, Michael J, Benjamin J Pasquale and Cyrus Samii. 2014. "Civil war and social cohesion: Lab-in-the-field evidence from Nepal." *American Journal of Political Science* 58(3):604–619.

Gleditsch, Kristian S and Michael D Ward. 1999. "A Revised List of Independent States Since the Congress of Vienna." *International Interactions* 25(4):393–413.

Goemans, Henk E, Kristian Skrede Gleditsch and Giacomo Chiozza. 2009. "Introducing Archigos: A Dataset of Political Leaders." *Journal of Peace Research* 46(2):269–283.

Goldsmith, Benjamin E. 2006. "A Universal Proposition? Region, Conflict, War and the Robustness of the Kantian Peace." *European Journal of International Relations* 12(4):533–563.

Green, Donald P and Alan S Gerber. 2015. *Get out the vote: How to increase voter turnout*. Brookings Institution Press.

Griffin, John D and Claudia Anewalt-Remsburg. 2013. "Legislator Wealth and the Effort to Repeal the Estate Tax." *American Politics Research* .

Hadzic, Dino, David Carlson and Margit Tavits. 2017. "How exposure to violence affects ethnic voting." *British journal of political science* pp. 1–18.

Hakhverdian, Armen. 2015. "Does it Matter that Most Representatives are Higher Educated?" *Swiss Political Science Review* 21(2):237–245.

Haslam, Nick. 2006. "Dehumanization: An Integrative Review." *Personality and Social Psychology Review* 10(3):252–264.

Hegre, Håvard and Nicholas Sambanis. 2006. "Sensitivity Analysis of Empirical Results on Civil War Onset." *Journal of Conflict Resolution* 50(4):508–535.

Henderson, Errol A. 2009. "Disturbing the Peace: African Warfare, Political Inversion and the Universality of the Democratic Peace Thesis." *British Journal of Political Science* 39(01):25–58.

Henry, PJ and David O Sears. 2009. "The crystallization of contemporary racial prejudice across the lifespan." *Political Psychology* 30(4):569–590.

Higgins, Edward Tory. 1996. "Knowledge activation: Accessibility, applicability, and salience.".

Hillygus, D Sunshine and Todd G Shields. 2014. *The persuadable voter: Wedge issues in presidential campaigns*. Princeton University Press.

Hirschman, Charles, Samuel Preston and Vu Manh Loi. 1995. "Vietnamese casualties during the American war: A new estimate." *Population and Development Review* 21(4):783–812.

Hong, Ji Yeon and Woo Chang Kang. 2015. "Trauma and stigma: The long-term effects of wartime violence on political attitudes." *Conflict Management and Peace Science* 34(3):264–286.

Horowitz, Donald L. 1985. *Ethnic groups in conflict*. University of California Press.

Horowitz, Jeremy and Kathleen Klaus. 2018. "Can Politicians Exploit Ethnic Grievances? An Experimental Study of Land Appeals in Kenya." *Political Behavior* pp. 1–24.

Horowitz, Michael C and Allan C Stam. 2014. "How Prior Military Experience Influences the Future Militarized behavior of leaders." *International Organization* 68(03):527–559.

Horowitz, Michael C, Allan C Stam and Cali M Ellis. 2015. *Why Leaders Fight*. Cambridge University Press.

Hout, Michael, Clem Brooks and Jeff Manza. 1995. "The Democratic Class Struggle in the United States, 1948-1992." *American Sociological Review* pp. 805–828.

Hutteman, Roos, Steffen Nestler, Jenny Wagner, Boris Egloff and Mitja D Back. 2015. "Wherever I May Roam: Processes of Self-esteem Development from Adolescence to Emerging Adulthood in the Context of International Student Exchange." *Journal of Personality and Social Psychology* 108(5):767.

Imai, Kosuke, Gary King, Clayton Nall et al. 2009. "The essential role of pair matching in cluster-randomized experiments, with application to the Mexican universal health insurance evaluation." *Statistical Science* 24(1):29–53.

Inglehart, Ronald. 1977. *The silent revolution: Changing values and political styles among Western publics*. Princeton, NJ: Princeton University Press.

Inglehart, Ronald. 1997. *Modernization and postmodernization: Cultural, economic, and political change in 43 societies*. Cambridge University Press.

Jennings, M Kent and Gregory B Markus. 1984. "Partisan Orientations Over the Long Haul: Results From the Three-wave Political Socialization Panel Study." *American Political Science Review* 78(04):1000–1018.

Jervis, Robert. 2013. "Do Leaders Matter and How Would We Know?" *Security Studies* 22(2):153–179.

Jones, Benjamin F, Benjamin A Olken et al. 2005. "Do Leaders Matter? National Leadership and Growth Since World War II." *The Quarterly Journal of Economics* 120(3):835–864.

Kagan, Donald. 1991. "Central Values are Central." *New York Times* .

Kalla, Joshua L and David E Broockman. 2018. "The minimal persuasive effects of campaign contact in general elections: Evidence from 49 field experiments." *American Political Science Review* 112(1):148–166.

Kern, Margaret L, Serenity S Della Porta and Howard S Friedman. 2014. "Lifelong Pathways to Longevity: Personality, Relationships, Flourishing, and Health." *Journal of Personality* 82(6):472–484.

Kijewski, Sara and Markus Freitag. 2018. "Civil War and the Formation of Social Trust in Kosovo Posttraumatic Growth or War-related Distress?" *Journal of Conflict Resolution* 62(4):717–742.

King, Gary, Michael Tomz and Jason Wittenberg. 2000. "Making the Most of Statistical Analyses: Improving Interpretation and Presentation." *American Journal of Political Science* pp. 347–361.

Kitschelt, Herbert and Philipp Rehm. 2014. "Occupations as a Site of Political Preference Formation." *Comparative Political Studies* 47(12):1670–1706.

Klar, Samara. 2014. "Partisanship in a social setting." *American Journal of Political Science* 58(3):687–704.

Klar, Samara and Yotam Shmargad. 2017. "The effect of network structure on preference formation." *The Journal of Politics* 79(2):717–721.

Knobler, Haim Y, H Knobler, E Cohen and MZ Abramowitz. 2016. "Lessons to be learned from the resilience and post traumatic growth of Holocaust survivors." *The European Journal of Public Health* 26(suppl 1):ckw171–042.

Kocher, Matthew Adam, Thomas B Pepinsky and Stathis N Kalyvas. 2011. "Aerial bombing and counterinsurgency in the Vietnam War." *American Journal of Political Science* 55(2):201–218.

Krosnick, Jon A and Duane F Alwin. 1989. "Aging and Susceptibility to Attitude Change." *Journal of Personality and Social Psychology* 57(3):416.

Kurzman, Charles. 2014. "World Values Lost in Translation.". *Washington Post*. September 2, online only.
URL: *https://www.washingtonpost.com/news/monkey-cage/wp/2014/09/02/world-values-lost-in-translation/*

Lake, David A and Donald Rothchild. 1996. "Containing fear: The origins and management of ethnic conflict." *International security* 21(2):41–75.

Lee, Loyd E. 1991. *World War Two: Crucible of the Contemporary World - Commentary and Readings: Crucible of the Contemporary World - Commentary and Readings*. New York, NY: Routledge.

Lelorain, S, A Bonnaud-Antignac and A Florin. 2010. "Long term posttraumatic growth after breast cancer: prevalence, predictors and relationships with psychological health." *Journal of Clinical Psychology in Medical Settings* 17(1):14–22.

Leonard, Wantchekon. 2004. "The paradox of "warlord" democracy: A theoretical investigation." *American Political Science Review* 98(01):17–33.

Lepič, Martin. 2017. "Limits to territorial nationalization in election support for an independence-aimed regional nationalism in Catalonia." *Political Geography* 60:190–202.

Lewis, Jeffrey. 2005. "The Janus Face of Brussels: Socialization and Everyday Decision Making in the European Union." *International Organization* 59(4):937.

Lewy, Guenter. 1978. *America in Vietnam*. New York, NY: Oxford University Press.

Linley, P Alex and Stephen Joseph. 2004. "Positive change following trauma and adversity: A review." *Journal of Traumatic Stress* 17(1):11–21.

Lipset, Seymour Martin. 1959. "Some social requisites of democracy: Economic development and political legitimacy." *American Political Science Review* 53(1):69–105.

Logevall, Fredrik. 2012. *Embers of war: The fall of an empire and the making of America's Vietnam*. Random House Incorporated.

Lupu, Noam. 2015. "Class and Representation in Latin America." *Swiss Political Science Review* 21(2):229–236.

Lupu, Noam and Leonid Peisakhin. 2017. "The Legacy of Political Violence across Generations." *American Journal of Political Science* .

Ma, Ying. 2007. "China's Stubborn Anti-democracy." *Policy Review* (141):3.

Mackie, Diane M and Eliot R Smith. 2002. *From Prejudice to Intergroup Emotions: Differentiated Reactions to Social Groups*. Psychology Press.

Malesky, Edmund J and Markus David Taussig. 2009. "Out of the gray: The impact of provincial institutions on business formalization in Vietnam." *Journal of East Asian Studies* 9:249–290.

Maoz, Zeev and Bruce Russett. 1993. "Normative and Structural Causes of Democratic Peace, 1946–1986." *American Political Science Review* 87(03):624–638.

Marcus, George E, W Russell Neuman and Michael MacKuen. 2000. *Affective intelligence and political judgment*. University of Chicago Press.

McDoom, Omar Shahabudin. 2012. "The psychology of threat in intergroup conflict: emotions, rationality, and opportunity in the Rwandan genocide." *International Security* 37(2):119–155.

Medeiros, Mike, Jean-Philippe Gauvin and Chris Chhim. 2015. "Refining vote choice in an ethno-regionalist context: Three-dimensional ideological voting in Catalonia and Quebec." *Electoral Studies* 40:14–22.

Merritt, Richard L. 1972. *Communication in International Politics*. University of Illinois Press.

Miguel, Edward and Gerard Roland. 2011. "The long-run impact of bombing Vietnam." *Journal of Development Economics* 96(1):1–15.

Mironova, Vera and Sam Whitt. 2016. "Social Norms after Conflict Exposure and Victimization by Violence: Experimental Evidence from Kosovo." *British Journal of Political Science* 48(3):1–17.

Montalvo, Jose G and Marta Reynal-Querol. 2005. "Ethnic Diversity and Economic Development." *Journal of Development Economics* 76(2):293–323.

Morris, Ian. 2014. *War! what is it Good For?: Conflict and the Progress of Civilization from Primates to Robots*. New York, NY: Farrar, Straus and Giroux.

Mousseau, Michael. 2000. "Market Prosperity, Democratic Consolidation, and Democratic Peace." *Journal of Conflict Resolution* 44(4):472–507.

Murthy, R Srinivasa and Rashmi Lakshminarayana. 2006. "Mental health consequences of war: a brief review of research findings." *World Psychiatry* 5(1):25–30.

Mutz, Diana C. 2018. "Status threat, not economic hardship, explains the 2016 presidential vote." *Proceedings of the National Academy of Sciences* p. 201718155.

Nations, United. 2013. "World population prospects: the 2012 revision." *Population Division of the Department of Economic and Social Affairs of the United Nations Secretariat* .

Neyer, Franz J and Judith Lehnart. 2007. "Relationships Matter in Personality Development: Evidence from an 8-year Longitudinal Study Across Young Adulthood." *Journal of Personality* 75(3):535–568.

Neyer, Franz J, Marcus Mund, Julia Zimmermann and Cornelia Wrzus. 2014. "Personality-Relationship Transactions Revisited." *Journal of Personality* 82(6):539–550.

Niemi, Richard G, Stephen C Craig and Franco Mattei. 1991. "Measuring internal political efficacy in the 1988 National Election Study." *American Political Science Review* 85(4):1407–1413.

Nørlund, Irene. 2007. "Civil Society in Vietnam. Social Organisations and Approaches to New Concepts." *ASIEN* 105:68–90.

Nunn, Nathan and Leonard Wantchekon. 2011. "The slave trade and the origins of mistrust in Africa." *The American Economic Review* 101(7):3221–3252.

Nye, Joseph S. 2004. *Soft Power: The Means to Success in World Politics*. Public Affairs.

Nye, Joseph S. 2008. "Public Diplomacy and Soft Power." *The Annals of the American Academy of Political and Social Science* 616(1):94–109.

Obermeyer, Ziad, Christopher JL Murray and Emmanuela Gakidou. 2008. "Fifty years of violent war deaths from Vietnam to Bosnia: analysis of data from the world health survey programme." *British Medical Journal* 336(7659):1482–1486.

Oliver, J Eric. 2000. "City size and civic involvement in metropolitan America." *American Political Science Review* 94(2):361–373.

Orriols, Lluis and Toni Rodon. 2016. "The 2015 Catalan election: the independence bid at the polls." *South European Society and Politics* 21(3):359–381.

Palmer, Glenn, Vito d'Orazio, Michael Kenwick and Matthew Lane. 2015. "The MID4 Dataset, 2002–2010: Procedures, Coding Rules and Description." *Conflict Management and Peace Science* 32(2):222–242.

Paluck, Elizabeth Levy. 2009. "Reducing intergroup prejudice and conflict using the media: a field experiment in Rwanda." *Journal of personality and social psychology* 96(3):574.

Paluck, Elizabeth Levy and Donald P Green. 2009. "Deference, dissent, and dispute resolution: An experimental intervention using mass media to change norms and behavior in Rwanda." *American Political Science Review* 103(4):622–644.

Pérez-Armendáriz, Clarisa and David Crow. 2009. "Do Migrants Remit Democracy? International Migration, Political Beliefs, and Behavior in Mexico." *Comparative Political Studies* 43:119–148.

Petersen, Roger D. 2002. *Understanding ethnic violence: Fear, hatred, and resentment in twentieth-century Eastern Europe*. Cambridge University Press.

Pettigrew, Thomas F and Linda R Tropp. 2006. "A Meta-analytic Test of Intergroup Contact Theory." *Journal of Personality and Social Psychology* 90(5):751.

Pop-Eleches, Grigore and Joshua A Tucker. 2017. *Communism's shadow: Historical legacies and contemporary political attitudes*. Princeton, NJ: Princeton University Press.

Posen, Barry R. 1993. "The security dilemma and ethnic conflict." *Survival* 35(1):27–47.

Posner, Daniel N. 2004. "The political salience of cultural difference: Why Chewas and Tumbukas are allies in Zambia and adversaries in Malawi." *American Political Science Review* 98(04):529–545.

Price, Vincent, David Tewksbury and Elizabeth Powers. 1997. "Switching trains of thought: The impact of news frames on readers' cognitive responses." *Communication research* 24(5):481–506.

Prorok, Alyssa K. 2016. "Leader Incentives and Civil War Outcomes." *American Journal of Political Science* 60(1):70–84.

Quaglia, Lucia, Fabrizio De Francesco and Claudio M Radaelli. 2008. "Committee Governance and Socialization in the European Union." *Journal of European Public Policy* 15(1):155–166.

Raudenbush, Stephen W, Andres Martinez and Jessaca Spybrook. 2007. "Strategies for improving precision in group-randomized experiments." *Educational Evaluation and Policy Analysis* 29(1):5–29.

Redish, Martin H and Kevin Finnerty. 2002. "What Did You Learn in School Today-Free Speech, Values Inculcation, and the Democratic-Educational Paradox." *Cornell Law Review* 88:62.

Reynal-Querol, Marta. 2002. "Ethnicity, Political Systems, and Civil Wars." *Journal of Conflict Resolution* 46(1):29–54.

Rico, Guillem and Robert Liñeira. 2014. "Bringing secessionism into the mainstream: The 2012 regional election in Catalonia." *South European Society and Politics* 19(2):257–280.

Robbins, Blaine G. 2012. "A Blessing and a Curse? Political Institutions in the Growth and Decay of Generalized Trust: A Cross-national Panel Analysis, 1980–2009." *PloS one* 7(4):e35120.

Rodon, Toni and Marc Guinjoan. 2018. "When the context matters: identity, secession and the spatial dimension in Catalonia." *Political Geography* 63:75–87.

Rosenfeld, Bryn, Kosuke Imai and Jacob N Shapiro. 2015. "An empirical validation study of popular survey methodologies for sensitive questions." *American Journal of Political Science* 60(3):783–802.

Ross, Marc Howard. 2007. *Cultural contestation in ethnic conflict*. Cambridge University Press.

Rozenas, Arturas, Sebastian Schutte and Yuri Zhukov. 2017. "The Political legacy of violence: The long-term impact of Stalin's repression in Ukraine." *Journal of Politics* 79(4):1147–1161.

Schmidt, Alexander W and Dennis C Spies. 2014. "Do parties "playing the race card" undermine natives' support for redistribution? Evidence from Europe." *Comparative Political Studies* 47(4):519–549.

Schuessler, Alexander A. 2000. *A logic of expressive choice*. Princeton, NJ: Princeton University Press.

Schulzinger, Robert D. 1997. *A time for war: the United States and Vietnam, 1941-1975*. New York, NY: Oxford University Press.

Sears, David O. 2002. "Long-term psychological consequences of political events." *Political Psychology* pp. 249–269.

Sears, David O and Carolyn L Funk. 1999. "Evidence of the Long-term Persistence of Adults' Political Predispositions." *The Journal of Politics* 61(01):1–28.

Selb, Peter and Simon Munzert. 2018. "Examining a Most Likely Case for Strong Campaign Effects: Hitler's Speeches and the Rise of the Nazi Party, 1927–1933." *American Political Science Review* pp. 1–17.

Shenon, Philip. 1995. "20 Years After Victory, Vietnamese Communists Ponder How to Celebrate." *New York Times* 23.

Simonovits, Gábor, Gábor Kézdi and Péter Kardos. 2018. "Seeing the World Through the Other's Eye: An Online Intervention Reducing Ethnic Prejudice." *American Political Science Review* 112(1):186–193.

Sinclair, Betsy. 2012. *The social citizen: Peer networks and political behavior*. Chicago, IL: University of Chicago Press.

Singer, J David. 1988. "Reconstructing the correlates of war dataset on material capabilities of states, 1816–1985." *International Interactions* 14(2):115–132.

Small, Dylan S. 2007. "Sensitivity analysis for instrumental variables regression with overidentifying restrictions." *Journal of the American Statistical Association* 102(479):1049–1058.

Spaiser, Viktoria, Shyam Ranganathan, Richard P Mann and David Sumpter. 2014. "The Dynamics of Democracy, Development and Cultural Values." *PloS one* 9(6).

Spilimbergo, Antonio. 2009. "Democracy and foreign education." *The American Economic Review* 99(1):528–543.

Stoeckel, Florian. 2016. "Contact and Community: The Role of Social Interactions for a Political Identity." *Political Psychology* 37(3):431–442.

Strack, Fritz, Leonard L Martin and Norbert Schwarz. 1988. "Priming and communication: Social determinants of information use in judgments of life satisfaction." *European journal of social psychology* 18(5):429–442.

Stroud, Natalie Jomini. 2008. "Media use and political predispositions: Revisiting the concept of selective exposure." *Political Behavior* 30(3):341–366.

Suvarierol, Semin, Madalina Busuioc and Martijn Groenleer. 2013. "Working for Europe? Socialization in the European Commission and agencies of the European Union." *Public Administration* 91(4):908–927.

Swers, Michele L. 2002. *The Difference Women Make: The Policy Impact of Women in Congress*. University of Chicago Press.

Taylor, William, Nguyn Thu Hng, Phm Quang Tú and Hunh Th Ngc Tuyt. 2012. "Civil society in Vietnam." *Hanoi: The Asia Foundation* .

Tedeschi, Richard G and Lawrence G Calhoun. 2004. "Posttraumatic growth: Conceptual foundations and empirical evidence." *Psychological Inquiry* 15(1):1–18.

Tilly, Charles and Gabriel Ardant. 1975. *The formation of national states in Western Europe, vol. 8*. Princeton, NJ: Princeton University Press.

Todd, Andrew R and Adam D Galinsky. 2014. "Perspective-taking as a strategy for improving intergroup relations: Evidence, mechanisms, and qualifications." *Social and Personality Psychology Compass* 8(7):374–387.

Tomz, Michael R and Jessica LP Weeks. 2013. "Public Opinion and the Democratic Peace." *American Political Science Review* 107(04):849–865.

Tsai, J, R El-Gabalawy, WH Sledge, SM Southwick and RH Pietrzak. 2015. "Post-traumatic growth among veterans in the USA: results from the National Health and Resilience in Veterans Study." *Psychological Medicine* 45(1):165–179.

Tucker, Spencer C. 2001. *Encyclopedia of the Vietnam War: A Political, Social, and Military History*. Abc-clio.

Vavreck, Lynn et al. 2007. "The exaggerated effects of advertising on turnout: The dangers of self-reports." *Quarterly Journal of Political Science* 2(4):325–343.

Visser, Penny S and Jon A Krosnick. 1998. "Development of Attitude Strength Over the Life Cycle: Surge and Decline." *Journal of Personality and Social Psychology* 75(6):1389.

Voors, Maarten J, Eleonora EM Nillesen, Philip Verwimp, Erwin H Bulte, Robert Lensink and Daan P Van Soest. 2012. "Violent conflict and behavior: A field experiment in Burundi." *The American Economic Review* 102(2):941–964.

Waltz, Kenneth N. 2010. *Theory of international politics*. Waveland Press.

Wantchekon, Leonard. 2003. "Clientelism and Voting Behavior: Evidence from a Field Experiment in Benin." *World Politics* 55(3):399–422.

Ward, Michael D, Brian D Greenhill and Kristin M Bakke. 2010. "The Perils of Policy by P-value: Pedicting Civil Conflicts." *Journal of Peace Research* 47(4):363–375.

Weart, Spencer R. 1998. *Never at War: Why Democracies Will Not Fight One Another*. Yale University Press.

Weeden, Kim A and David B Grusky. 2005. "The Case for a New Class Map 1." *American Journal of Sociology* 111(1):141–212.

Weeks, Jessica L. 2012. "Strongmen and straw men: Authoritarian regimes and the initiation of international conflict." *American Political Science Review* 106(2):326–347.

Weidmann, Nils B, Doreen Kuse and Kristian Skrede Gleditsch. 2010. "The Geography of the International System: The CShapes Dataset." *International Interactions* 36(1):86–106.

Weidmann, Nils B and Kristian Gleditsch. 2010. "Mapping and Measuring Country Shapes: The CShapes Package." *R Journal* 2(1):18–23.

Welzel, Christian. 2013*a*. *Freedom rising*. Cambridge, MA: Cambridge University Press.

Welzel, Christian. 2013*b*. *Freedom rising: Human Empowerment and the Quest for Emancipation*. New York, NY: Cambridge University Press.

Welzel, Christian and Franziska Deutsch. 2012. "Emancipative Values and Non-violent Protest: The Importance of 'Ecological' Effects." *British Journal of Political Science* 42(02):465–479.

Welzel, Christian, Ronald F Inglehart and Stefan Kruse. 2017. "Pitfalls in the Study of Democratization." *British Journal of Political Science* 47(2):463–472.

Welzel, Christian and Ronald Inglehart. 2008. "The Role of Ordinary People in Democratization." *Journal of Democracy* 19(1):126–140.

Wielhouwer, Peter W. 2003. "In Search of Lincoln's Perfect List: Targeting in Grassroots Campaigns." *American Politics Research* 31(6):632–669.

Wilkinson, Steven I. 2006. *Votes and violence: Electoral competition and ethnic riots in India*. Cambridge University Press.

Wischermann, Jörg. 2013. "Civic organizations in Vietnam's one-party state: Supporters of authoritarian rule?" *GIGA Working Paper* 228.

Wood, Elisabeth Jean. 2003. *Insurgent collective action and civil war in El Salvador*. New York, NY: Cambridge University Press.

World Bank. 2011. *World development report 2011: conflict, security, and development*. World Bank.

Ying, Yu-Wen. 2002. "The Effect of Cross-cultural Living on Personality: Assimilation and Accommodation Among Taiwanese Young Adults in the United States." *American Journal of Orthopsychiatry* 72(3):362.

Zimmermann, Julia and Franz J Neyer. 2013. "Do We Become a Different Person When Hitting the Road? Personality Development of Sojourners." *Journal of Personality and Social Psychology* 105(3):515.

Appendix A

Are Western-Educated Leaders Less Prone to Initiate Militarized Disputes?

A.1 List of non-Western countries included in the main analysis

The list of 147 non-Western countries that are included in the analyses throughout the paper is the following:

Afghanistan, Albania, Algeria, Angola, Argentina, Armenia, Azerbaijan, Bahrein, Barbados, Benin, Burkina Faso, Bahamas, Bhutan, Belarus, Belize, Bangladesh, Bolivia, Bosnia-Herzegovina, Botswana, Brazil, Brunei, Burundi, Bulgaria, Cambodia, Cameroon, Cape Verde, Ivory Coast, Central African Republic, Chad, Chile, China, Colombia, Comoros, Republic of Congo, Costa Rica, Croatia, Cuba, Cyprus, Chech Republic, Djibouti, Dominican Republic, Congo (DRC), Vietnam, Ecuador, Egypt, Equatorial Guinea, Eritrea, Estonia, Ethiopia, Fiji, Gabon, Gambia, Ghana, Guinea-Bissau, Georgia, Guatemala, Republic of Guinea, Guayana, Haiti, Honduras, Hungary, India, Indonesia, Iran, Iraq, Jamaica, Jordan, Kenya, Kuwait, Kyrgyzstan, Kazakhstan, Laos, Latvia, Liberia, Lebanon, Lesotho, Libya, Lithuania, Mauritania, Macedonia, Madagascar, Malaysia, Mauritius, Malawi, Mexico, Moldova, Mali, Malta, Mongolia, Morocco, Myanmar, Mozambique, Namibia, Nepal, Nicaragua, Nigeria, Niger, Oman, Pakistan, Panama, Paraguay, Peru, Philippines, Papua New Guinea, Poland, Korea (DR), Qatar, Korea R., Romania, Russian Federation/USSR, Vietnam N., Rwanda, South Africa, El Salvador, Saudi Arabia, Senegal, Sierra Leone, Singapore, Slovakia, Slovenia, Somalia, Sri Lanka, Sudan, Swziland, Syria, Tajikistan, Taiwan, Tanzania, Thailand, Turkmenistan, Togo, Trinidad and Tobago, Tunisia, Turkey, United Arab Emirates, Uganda, Ukraine, Uruguay, Uzbekistan, Venezuela, Yemen Arab Republic/North Yemen, Republic of Yemen, South Yemen, Yugoslavia/Serbia, Zambia, Zanzibar, Zimbabwe.

A.2 Robustness Checks: Militarized Interstate Disputes Initiated by the leader only

The next table re-estimates the main finding but taking only into account those militarized interstate disputes that were initiated by the leader, so excluding inherited wars.

Table A.1: The Effect of Western-Democratic Education on Militarized Interstate Disputes Initiated by the Leader Only

	Dependent variable: Interstate Dispute Initiation					
	(1)	(2)	(3)	(4)	(5)	(6)
Western Education	−1.16***	−0.89***	−0.79***	−0.81***	−0.76***	−0.57**
	(0.24)	(0.21)	(0.23)	(0.21)	(0.23)	(0.21)
Leader Controls						
Secondary Studies			0.61		0.70	0.81
			(0.51)		(0.51)	(0.50)
Undergraduate			0.61		0.27	0.55
			(0.48)		(0.48)	(0.46)
Post-graduate			0.59		0.70	0.62
			(0.51)		(0.51)	(0.48)
Foreign education			−0.33		−0.26	−0.41
(non-Western)			(0.27)		(0.26)	(0.25)
Top University			0.07		0.14	0.45
			(0.35)		(0.35)	(0.32)
Prior occupation dummies?	N	N	Y	N	Y	Y
Country Controls						
Democracy Score				−0.03	−0.03	−0.01
				(0.02)	(0.02)	(0.02)
GDPpc				−0.35***	−0.35**	−0.32**
				(0.11)	(0.11)	(0.14)
Last War Won				0.76*	0.77*	0.85***
				(0.37)	(0.38)	(0.32)
Last War Lost				0.32	0.25	0.12
				(0.33)	(0.35)	(0.31)
Material Capabilities				19.7***	19.8**	−8.33
				(5.78)	(5.81)	(8.41)
Student Flow (000')				0.08***	0.07**	0.01
				(0.02)	(0.02)	(0.02)
Ethnic Fractionalization				0.04	0.06	
				(0.13)	(0.13)	
Colonial legacy				−0.62	−0.63	
				(0.32)	(0.33)	
Distance to the West (000')				0.02	0.03	
				(0.01)	(0.07)	
Random/Fixed Effects						
Leader RE	Y	Y	Y	Y	Y	Y
Year RE/FE	N	FE	FE	FE	FE	FE
Country RE/FE	N	RE	RE	RE	RE	FE
Constant	−2.38***	−3.29***	−3.27***	−0.72	−1.31	−0.41
	(0.12)	(0.58)	(0.70)	(0.93)	(1.07)	(1.27)
N	6,209	6,209	6,209	6,209	6,209	5,258
N Countries	147	147	147	147	147	111
N Years	55	55	55	55	55	55
N Leaders	902	902	902	902	902	744
LL	−2,329	−2,210	−2,205	−2,188	−2,184	−2,024
AIC	4,664	4,536	4,553	4,510	4,528	4,420
BIC	4,684	4,926	5,032	4,961	5,066	5,642

Note: *p<0.05; **p<0.01; ***p<0.001. Models are logistic mixed effects. GPDpc, ethnic fractionalization and student flow are included in their logarithmic scale since this is the most appropriate functional form in the relationship between these variables and the outcome variable.

A.3 Robustness Checks: Coding sensitivity of Western education

The next three table re-estimate the main finding after re-coding Western education by: 1) including in the definition of Western-based democratic education those non-Western countries that have been democratic throughout the entire period (Table A.2); 2) excluding in the definition of Western-based democratic education those countries that have not been democratic throughout the entire period (Table A.3); and, 3) by including in the definition of Western-based democratic education only those non-Western countries that have been democratic throughout the entire period and, also, excluding in the definition of Western-based democratic education those Western countries that have not been democratic throughout the entire period (Table A.4). See footnote 12 in the main text for greater details on the countries. The conclusion across these different specification is that results are largely unaltered by defining the countries as Western (Table 1.1 in main body of the article), Western or democratic (Table A.2), Western-democratic (Table A.3), or only Democratic (Table A.4). This consistency supports the thesis that Western countries and democratic regime types have been historically too closely connected to empirically distinguish them in these analyses.

Table A.2: The Effect of Western-Democratic Education on Militarized Interstate Disputes (Western and/or Democratic Education)

	Dependent variable: Interstate Dispute Initiation					
	(1)	(2)	(3)	(4)	(5)	(6)
Western Education	−1.28***	−0.91***	−0.86***	−0.83***	−0.84***	−0.80***
	(0.26)	(0.20)	(0.22)	(0.20)	(0.22)	(0.23)
Leader Controls						
Secondary Studies			0.60		0.68	0.68
			(0.50)		(0.50)	(0.49)
Undergraduate			0.48		0.57	0.42
			(0.47)		(0.46)	(0.44)
Post-graduate			0.64		0.74	0.55
			(0.50)		(0.49)	(0.47)
Foreign education			−0.13		−0.08	−0.19
(non-Western)			(0.25)		(0.25)	(0.24)
Top University			0.07		0.15	0.43
			(0.34)		(0.33)	(0.31)
Prior occupation dummies?	N	N	Y	N	Y	Y
Country Controls						
Democracy Score				−0.03	−0.03	−0.01
				(0.01)	(0.01)	(0.01)
GDPpc				−0.38***	−0.38**	−0.40**
				(0.11)	(0.11)	(0.14)
Last War Won				0.69*	0.73*	0.78*
				(0.35)	(0.37)	(0.32)
Last War Lost				0.30	0.25	0.14
				(0.31)	(0.33)	(0.29)
Material Capabilities				21.8***	22.0***	−3.55
				(5.96)	(5.99)	(8.38)
Student Flow (000')				0.08***	0.08***	0.13
				(0.02)	(0.02)	(0.02)
Ethnic Fractionalization				0.08	0.06	
				(0.11)	(0.13)	
Colonial legacy				−0.61	−0.52	
				(0.33)	(0.33)	
Distance to the West (000')				0.02	0.02	
				(0.07)	(0.14)	
Random/Fixed Effects						
Leader RE	Y	Y	Y	Y	Y	Y
Year RE/FE	N	FE	FE	FE	FE	FE
Country RE/FE	N	RE	RE	RE	RE	FE
Constant	−2.23***	−3.29***	−3.75***	−0.56	−1.08	−0.41
	(0.12)	(0.58)	(0.74)	(0.93)	(1.05)	(1.27)
N	6,209	6,209	6,209	6,209	6,209	5,258
N Countries	147	147	147	147	147	111
N Years	55	55	55	55	55	55
N Leaders	902	902	902	902	902	744
LL	−2,385	−2,236	−2,221	−2,216	−2,213	−2,042
AIC	4,776	4,595	4,614	4,566	4,586	4,457
BIC	4,797	4,985	5,091	5,018	5,125	5,678

Note: *p<0.05; **p<0.01; ***p<0.001. Models are logistic mixed effects. GPDpc, ethnic fractionalization and student flow are included in their logarithmic scale since this is the most appropriate functional form in the relationship between these variables and the outcome variable.

Table A.3: The Effect of Western-Democratic Education on Militarized Interstate Disputes (Democratic Education)

	Dependent variable: Interstate Dispute Initiation					
	(1)	(2)	(3)	(4)	(5)	(6)
Western Education	−1.23***	−1.05***	−0.99***	−0.94***	−0.91***	−0.82***
	(0.27)	(0.24)	(0.25)	(0.24)	(0.20)	(0.23)
Leader Controls						
Secondary Studies			0.55		0.64	0.68
			(0.50)		(0.50)	(0.48)
Undergraduate			0.42		0.51	0.42
			(0.47)		(0.47)	(0.44)
Post-graduate			0.53		0.62	0.55
			(0.49)		(0.49)	(0.47)
Foreign education			−0.16		−0.10	−0.18
(non-Western)			(0.25)		(0.25)	(0.24)
Top University			0.11		0.20	0.42
			(0.33)		(0.33)	(0.31)
Prior occupation dummies?	N	N	Y	N	Y	Y
Country Controls						
Democracy Score				−0.02	−0.03	−0.01
				(0.01)	(0.01)	(0.01)
GDPpc				−0.37***	−0.36***	−0.40**
				(0.11)	(0.11)	(0.14)
Last War Won				0.71*	0.73	0.78*
				(0.35)	(0.37)	(0.32)
Last War Lost				0.31	0.24	0.13
				(0.31)	(0.33)	(0.29)
Material Capabilities				21.8***	21.98***	−3.63
				(6.00)	(6.04)	(8.38)
Student Flow (000')				0.08***	0.08***	0.01
				(0.02)	(0.02)	(0.02)
Ethnic Fractionalization				0.07	0.08	
				(0.11)	(0.11)	
Colonial legacy				−0.65	−0.67	
				(0.33)	(0.34)	
Distance to the West				0.02	0.02	
				(0.07)	(0.07)	
Random/Fixed Effects						
Leader RE	Y	Y	Y	Y	Y	Y
Year RE/FE	N	FE	FE	FE	FE	FE
Country RE/FE	N	RE	RE	RE	RE	FE
Constant	−2.33***	−3.33***	−3.72***	−0.68	−1.15	−0.41
	(0.12)	(0.58)	(0.74)	(0.93)	(1.05)	(1.27)
N	6,209	6,209	6,209	6,209	6,209	5,258
N Countries	147	147	147	147	147	111
N Years	55	55	55	55	55	55
N Leaders	902	902	902	902	902	744
LL	−2,390	−2,240	−2,235	−2,216	−2,213	−2,042
AIC	4,786	4,595	4,613	4,567	4,588	4,456
BIC	4,806	4,896	5,091	5,019	5,126	5,678

Note: *p<0.05; **p<0.01; ***p<0.001. Models are logistic mixed effects. GPDpc, ethnic fractionalization and student flow are included in their logarithmic scale since this is the most appropriate functional form in the relationship between these variables and the outcome variable.

Table A.4: The Effect of Western-Democratic Education on Militarized Interstate Disputes (Western and Democratic Education)

	Dependent variable: Interstate Dispute Initiation					
	(1)	(2)	(3)	(4)	(5)	(6)
Western Education	−1.27***	−1.08***	−1.02***	−0.98***	−0.94***	−0.82**
	(0.27)	(0.24)	(0.22)	(0.23)	(0.24)	(0.23)
Leader Controls						
Secondary Studies			0.55		0.64	0.68
			(0.50)		(0.50)	(0.48)
Undergraduate			0.42		0.51	0.42
			(0.47)		(0.47)	(0.44)
Post-graduate			0.54		0.63	0.55
			(0.49)		(0.49)	(0.47)
Foreign education			−0.15		−0.09	−0.18
(non-Western)			(0.25)		(0.25)	(0.24)
Top University			0.09		0.19	0.42
			(0.33)		(0.33)	(0.31)
Prior occupation dummies?	N	N	Y	N	Y	Y
Country Controls						
Democracy Score				−0.03	−0.03	−0.01
				(0.01)	(0.01)	(0.01)
GDPpc				−0.34***	−0.36**	−0.40**
				(0.11)	(0.11)	(0.14)
Last War Won				0.70*	0.73*	0.78*
				(0.35)	(0.37)	(0.32)
Last War Lost				0.31	0.23	0.13
				(0.31)	(0.33)	(0.29)
Material Capabilities				23.7***	21.9***	−3.62
				(5.95)	(6.03)	(8.38)
Student Flow (000')				0.08***	0.08***	0.01
				(0.02)	(0.02)	(0.02)
Ethnic Fractionalization				0.04	0.08	
				(0.11)	(0.13)	
Colonial legacy				−0.51	−0.67*	
				(0.32)	(0.34)	
Distance to the West				0.02	0.02	
				(0.14)	(0.07)	
Random/Fixed Effects						
Leader RE	Y	Y	Y	Y	Y	Y
Year RE/FE	N	FE	FE	FE	FE	FE
Country RE/FE	N	RE	RE	RE	RE	FE
Constant	−2.32***	−3.29***	−3.71***	−1.32	−1.17	−1.321
	(0.12)	(0.58)	(0.70)	(0.93)	(1.05)	(1.27)
N	6,209	6,209	6,209	6,209	6,209	5,258
N Countries	147	147	147	147	147	111
N Years	55	55	55	55	55	55
N Leaders	902	902	902	902	902	744
LL	−2,389	−2,239	−2,235	−2,218	−2,213	−2,042
AIC	4,784	4,594	4,612	4,565	4,586	4,456
BIC	4,805	4,984	5,090	4,995	5,125	5,678

Note: *p<0.05; **p<0.01; ***p<0.001. Models are logistic mixed effects. GPDpc, ethnic fractionalization and student flow are included in their logarithmic scale since this is the most appropriate functional form in the relationship between these variables and the outcome variable.

A.4 Alternative Matching Approach: Hybrid Method, Nearest and Exact Matching

This Appendix presents the results for an alternative matching procedure that combines a nearest neighbor matching for continuous variables and an exact matching for categorical variables. While this procedure allows for exact matching on some key predictors, the matching on continuous covariates is less perfect than in the procedure that uniquely matches through a nearest neighbor. Additionally, the number of observations that can be matched here are substantially lower than those observations that are used in the nearest neighbor approach. Therefore, I present the nearest neighbor approach in the main text because I believe that it is a superior technique for the analysis of this dataset. Yet, I also report the hybrid approach below:

Table A.5: The Effect of Western Education on War Initiation (Hybrid Matching Approach, Nearest and Exact)

	Dependent variable: Interstate Dispute Initiation			
	OLS	*Logistic Regression Models*		
Western education	−0.05***	−0.73***	−0.77***	−1.04***
	(0.02)	(0.24)	(0.22)	(0.29)
Intercept	0.40	1.39	1.05	33.43
	(0.02)	(0.91)	(1.38)	(35.37)
Controls	Y	Y	Y	Y
Year FE	N	N	Y	Y
Country FE	N	N	N	Y
N Treatment Group	861	861	861	456
N Control Group	861	861	861	562
N Total	1,722	1,722	1,722	1,018
Balance of Covariates	**Treatment Group**	**Control Group**	**Control Group**	**Treatment Group**
	Means/proportions	Means/proportions	Standard deviation	Diff. in means/proportions
Level of Education	2.32	2.32	0.76	0.000
Businesspeople	0.087	0.087	0.282	0.000
Gentry	0.105	0.105	0.307	0.000
Blue-collar worker	0.064	0.064	0.245	0.000
Military	0.180	0.180	0.384	0.000
Lawyers	0.287	0.287	0.453	0.000
Religious	0.000	0.000	0.000	0.000
Scientists	0.053	0.053	0.225	0.000
Service	0.329	0.329	0.0.47	0.000
Democracy Score	0.24	1.19	6.67	-0.95
GDPpc	7.08	7.12	1.39	-0.039
Last War Won	0.007	0.007	0.0.08	0.00
Last War Lost	0.02	0.02	0.15	0.00
Material Capabilities	0.001	0.003	0.007	-0.002
Student Flow (000')	-.208	0.448	3.63	-0.65
Ethnic Fractionalization	-0.777	0.916	-1.35	0.16
Colonial Legacy	0.863	0.863	0.344	0.000
Distance to the West	3,954	4,247	2,289	-292.9

Note: *p<0.1; **p<0.05; ***p<0.01. Models report cluster-robust standard error by leader to correct for the within-leader correlation of observations. The matching procedure is nearest neighbor. See the online Appendix for a full report of the balance of the covariates in the unmatched dataset compared to the balance of the covariates in the matched dataset. Just as for all the models in Table 1.1, only non-Western leaders are used for the analysis. Controls "Y" means that all controls that are shown in the list of Balance of Covariates are also included in the regression models. Because the matching is not exact, the inclusion of the controls in the post-matching analysis adjusts for the remaining differences in the value of the covariates between the treated and the control units. Yet, the decision of including the set of controls in the post-matching analysis does not change any of the results presented here. Year FE and Country FE are year and country fixed effects. The last column drops observations that appear only once within a country.

A.5 Robustness Checks: Country-Specific Time Trends

This Appendix presents the results for an alternative way to adjust for smooth country-level trends in unobserved confounders by including linear, quadratic, and cubic country specific time trends into the model. This procedure is suggested by (Carter and Signorino, 2010). The main finding of the paper is unaffected by the inclusion of these time trends. Yet, I believe that the inclusion of year dummies, which controls for common international shocks, is the most appropriate form to control for time due to the nature of the dependent variable. Note that it is more reasonable to believe that the likelihood of countries to get involved in war is a function of the international environment to a specific year – which is shared by most other countries – rather than a function of time from which the country emerged as an independent nation state in the sample. In other words, the likelihood that a country will be involved in a war in, say, 1970, is much more affected by international events in that year, say, the Second Indochina War, just to mention an active war in that year, than by the fact that that country has been an independent state for x number of years, regardless of the functional form given to time.

Table A.6: The Effect of Western-educated Leaders on Militarized Interstate Disputes

	Dependent variable: Interstate Dispute Initiation					
	(1)	(2)	(3)	(4)	(5)	(6)
Western Education	−1.24***	−0.88***	−0.84***	−0.80***	−0.81***	−0.65**
	(0.24)	(0.20)	(0.21)	(0.20)	(0.21)	(0.19)
Leader Controls						
Secondary Studies			0.65		0.72	0.81
			(0.50)		(0.49)	(0.47)
Undergraduate			0.52		0.60	0.54
			(0.46)		(0.46)	(0.44)
Post-graduate			0.65		0.75	0.67
			(0.49)		(0.48)	(0.46)
Foreign education			−0.10		−0.05	−0.13
(non-Western)			(0.25)		(0.24)	(0.23)
Top University			0.04		0.12	0.38
			(0.33)		(0.33)	(0.30)
Prior occupation dummies?	N	N	Y	N	Y	Y
Country Controls						
Democracy Score				−0.03	−0.03	−0.02
				(0.01)	(0.02)	(0.01)
GDPpc				−0.37***	−0.36***	−0.38**
				(0.10)	(0.11)	(0.13)
Last War Won				0.66*	0.72*	0.79*
				(0.34)	(0.36)	(0.31)
Last War Lost				0.27	0.24	0.12
				(0.30)	(0.32)	(0.29)
Material Capabilities				22.5***	22.8***	−1.35
				(5.83)	(5.86)	(8.10)
Student Flow (000')				0.07***	0.07***	0.01
				(0.02)	(0.02)	(0.02)
Ethnic Fractionalization				0.08	0.06	
				(0.10)	(0.13)	
Colonial legacy				−0.58	−0.52	
				(0.33)	(0.33)	
Distance to the West				0.01	0.02	
				(0.07)	(0.14)	
Random/Fixed Effects						
Leader RE	Y	Y	Y	Y	Y	Y
Time Trends	Y	Y	Y	Y	Y	Y
Country RE/FE	N	RE	RE	RE	RE	FE
Constant	−2.23***	−2.88***	−3.36***	−0.40	−1.23	−0.41
	(0.12)	(0.34)	(0.57)	(0.79)	(0.74)	(1.27)
N	6,209	6,209	6,209	6,209	6,209	5,258
N Countries	147	147	147	147	147	111
N Years	55	55	55	55	55	55
N Leaders	902	902	902	902	902	744
LL	−2,385	−2,292	−2,288	−2,267	−2,266	−2,095
AIC	4,776	4,598	4,617	4,566	4,589	4,461
BIC	4,797	4,645	4,751	4,673	4,784	5,347

Note: *p<0.05; **p<0.01; ***p<0.001. Models are logistic mixed effects. GPDpc, ethnic fractionalization and student flow are included in their logarithmic scale since this is the most appropriate functional form in the relationship between these variables and the outcome variable. Following (Carter and Signorino, 2010), time trends incorporate linear, quadratic, and cubic country specific time trends to account for smooth country trends in unobserved confounders.

Appendix B

The Long-Term Effects of War on Civic Engagement: Evidence from the Vietnam War

B.1 Descriptive statistics (survey data)

Table B.1: Descriptive statistics (survey data)

	Mean	(SD)	$Min \rightarrow Max$	Loadings	NA (%)
Panel A: Demographics					
Age	42.5	(15.7)	$18 \rightarrow 96$	–	< 0.01
Gender	0.51	(0.50)	$0 \rightarrow 1$	–	0.01
Education	4.4	(1.9)	$1 \rightarrow 9$	–	0.00
Panel B: Collective Action (voluntary work)					
Welfare services org.	0.29	(0.45)	$0 \rightarrow 1$	0.67	0.00
Religious org.	0.10	(0.29)	$0 \rightarrow 1$	0.33	0.00
Cultural org.	0.16	(0.36)	$0 \rightarrow 1$	0.75	0.00
Labor Unions	0.10	(0.30)	$0 \rightarrow 1$	0.65	0.00
Political groups or organizations	0.24	(0.43)	$0 \rightarrow 1$	0.62	0.00
Community action (e.g. poverty, unemployment)	0.26	(0.44)	$0 \rightarrow 1$	0.75	0.00
Human rights	0.01	(0.11)	$0 \rightarrow 1$	0.61	0.00
Conservation or environmental org.	0.08	(0.27)	$0 \rightarrow 1$	0.78	0.00
Professional assoc.	0.10	(0.31)	$0 \rightarrow 1$	0.52	0.00
Youth work (scouts, guides, etc.)	0.14	(0.35)	$0 \rightarrow 1$	0.57	0.00
Sports or recreation	0.18	(0.39)	$0 \rightarrow 1$	0.65	0.00
Women's groups	0.27	(0.44)	$0 \rightarrow 1$	0.30	0.00
Peace movements	0.07	(0.25)	$0 \rightarrow 1$	0.88	0.00
Health org.	0.15	(0.36)	$0 \rightarrow 1$	0.71	0.00
Factor social activity (log scale)	-0.17	(0.53)	$-0.6 \rightarrow 1.7$	–	0.00
Panel C: Collective Action (membership)					
Welfare services org.	0.27	(0.44)	$0 \rightarrow 1$	0.65	0.00
Religious org.	0.10	(0.31)	$0 \rightarrow 1$	0.36	0.00
Cultural org.	0.17	(0.38)	$0 \rightarrow 1$	0.73	0.00

Continued on next page

Table B.1 – continued from previous page

	Mean	(SD)	$Min \to Max$	Loadings	NA (%)
Labor Unions	0.11	(0.32)	$0 \to 1$	0.68	0.00
Political groups or organizations	0.29	(0.45)	$0 \to 1$	0.66	0.00
Community action (e.g. poverty, unemployment)	0.26	(0.44)	$0 \to 1$	0.72	0.00
Human rights	0.02	(0.12)	$0 \to 1$	0.69	0.00
Conservation or environmental org.	0.08	(0.27)	$0 \to 1$	0.84	0.00
Professional assoc.	0.13	(0.34)	$0 \to 1$	0.53	0.00
Youth work (scouts, guides, etc.)	0.15	(0.36)	$0 \to 1$	0.56	0.00
Sports or recreation	0.19	(0.39)	$0 \to 1$	0.65	0.00
Women's groups	0.28	(0.45)	$0 \to 1$	0.34	0.00
Peace movements	0.09	(0.29)	$0 \to 1$	0.86	0.00
Health org.	0.15	(0.36)	$0 \to 1$	0.80	0.00
Factor social membership (log scale)	0.62	0.33	$-1.0 \to 1.8$	–	0.00
Panel D: Participatory Values					
Block 1 items	0.36	(0.31)	$0 \to 1$	–	0.00
Block 2 items	0.21	(0.34)	$0 \to 1$	–	0.00
Index of Participatory Values	0.29	(0.26)	$0 \to 1$	–	0.00

B.2 Descriptive statistics (province data)

Table B.2: Descriptive statistics (province data)

	Province-level data		
	Mean	(SD)	Min → Max
Bombing intensity			
Total # ordnance (per km^2)	28.63	49.73	0.01 → 335.47
Controls			
Average precipitation	156.47	30.89	100.42 → 250.17
Population density (1960-61)	251.2	484.0	9 → 2,868
Latitude	18.0	5.41	10.01 → 25.19
South	0.49	0.51	0 → 1

B.3 Correlation Matrix of Different Types of Ordnance

Table B.3: Correlation matrix of different types of ordnance (province-level)

	Total ordnance	General Purpose	Cluster Bombs	Missiles	Rockets	Cannon Artillery	Incendiary	White Phosphorus
Total # ordnance								
General Purpose bombs	1.00***							
Cluster Bombs	0.74***	0.72***						
Missiles	0.76***	0.77***	0.47***					
Rockets	0.72***	0.70***	0.83***	0.53***				
Cannon Artillery	0.34**	0.32*	0.52***	0.04	0.21			
Incendiary bombs	0.65***	0.62***	0.67***	0.31*	0.57***	0.48***		
White Phosphorus	0.53***	0.53***	0.48***	0.38**	0.54***	0.11	0.34**	
Ammunition ('000)	0.47***	0.43***	0.57***	0.09	0.47***	0.49***	0.88***	0.28*

Note: All values in the table are standardized by geographic scale, per km^2. Significance levels: * (p<0.05), ** (p<0.01), *** (p<0.001).

B.4 Question Wordings

I report here the question wording for the questions from the World Value Survey 2001 used in the analysis.

B.4.1 Question Wordings: Collective Action (membership and activity)

[Membership] Please look carefully at the following list of voluntary organizations and activities and say which, if any, do you belong to? (Code all 'yes' answers as 1, if not mentioned code as 2).

[Activity] And for which, if any, are you currently doing unpaid voluntary work (Code all 'yes' answers as 1, if not mentioned code as 2):

1) Social welfare services for elderly, handicapped or deprived people; 2) Religious organizations; 3) Education, arts, music or cultural activities; 4) Labor Unions; 5) Political groups or organizations; 6) Local community action on issues like poverty, employment, housing, racial equality; 7) Third world development or human rights; 8) Conservation, environment, animal rights groups; 9) Professional associations; 10) Youth work (e.g. scouts, guides, youth clubs etc.); 11) Sports or recreation; 12) Women's groups; 13) Peace movement; 14) Voluntary organisations concerned with health; and, 15) Other groups (not used in the analysis).

B.4.2 Question Wordings: Participatory Values

"People sometimes talk about what the aims of this country should be for the next ten years. On this card are listed some of the goals which different people would give top priority. Would

you please say which one of these you, yourself, consider the most important? (...) And second most important?"

There are twelve aims in total, organized in three four-item batteries, with two post-materialist items in each battery. Of these six post-materialist items, three measure an emphasis on people's participatory values. Table B.4 reports the exact wording of the choices that appeared in the survey with the measures that emphasize people's voice in cursive and bold. Hence, only the answers in questions V120-V124 are used to create the Voice Index. Yet, because Block 1 contains one voice item and Block 2 contains two voice items, the coding slightly differs across the questions.

The coding procedure used here strictly follows the coding instructions provided by (Welzel, 2013*b*, 66-69) and is imposed by the way in which the WVS asks these questions. Notice that the coding differs across the items within the two blocks questions because Block 1 contains only one voice item and Block 2 contains two voice items.

In Block 1, responses are recoded into 0 when the item has not been chosen as important, 0.5 when it has been chosen as second most important and 1 when it has been chosen as most important. If both V120 and V121 have a value of 9, then the answer is 0. Finally, if no response was provided, then the answer is recoded as a missing value.

In Block 2, responses are, first, recoded as 1 if the two voice responses are chosen as first and second, regardless of the chosen order. Secondly, if one of the two responses is chosen as the first choice, but the other is not chosen as the second choice, then the answer is coded as 0.66. Thirdly, if one of the two responses is chosen as the second choice, but the other is not chosen as the first choice, then the answer is coded as 0.33. Finally, if none of the two voice items is chosen, then the answer is coded as 0. If both V120 and V121 have a value of 9, then

the answer is 0; yet, if only one of the two items is 9, then the item with a 9 is treated as if it were a 0. Finally, if no response was provided, then the answer is recoded as a missing value.

To create the index of participatory values, the recoded scores are averaged over the scores from the two blocks.

Table B.4: Wording of the answer key to create the Index of Participatory Values

Block 1 [question code: V120-121]	**1ST CHOICE**	**2ND CHOICE**
A high level of economic growth	1	1
Making sure this country has strong defense forces	2	2
Seeing that people have more say about how things are done at their jobs and in their communities	3	3
Trying to make our cities and countryside more beautiful	4	4
Don't know [DON'T BE READ OUT]	9	9
Block 2 [question code: V122-123]	**1ST CHOICE**	**2ND CHOICE**
Maintaining order in the nation	1	1
Giving people more say in important government decisions	2	2
Fighting rising prices	3	3
Protecting freedom of speech	4	4
Don't know [DON'T BE READ OUT]	9	9
Block 3 [question code: V124-125]	**1ST CHOICE**	**2ND CHOICE**
A stable economy	1	1
Progress toward a less impersonal and more humane society	2	2
Progress toward a society in which ideas count more than money	3	3
The fight against crime	4	4
Don't know [DO NOT READ OUT]	9	9

B.5 Number of Respondents by Province

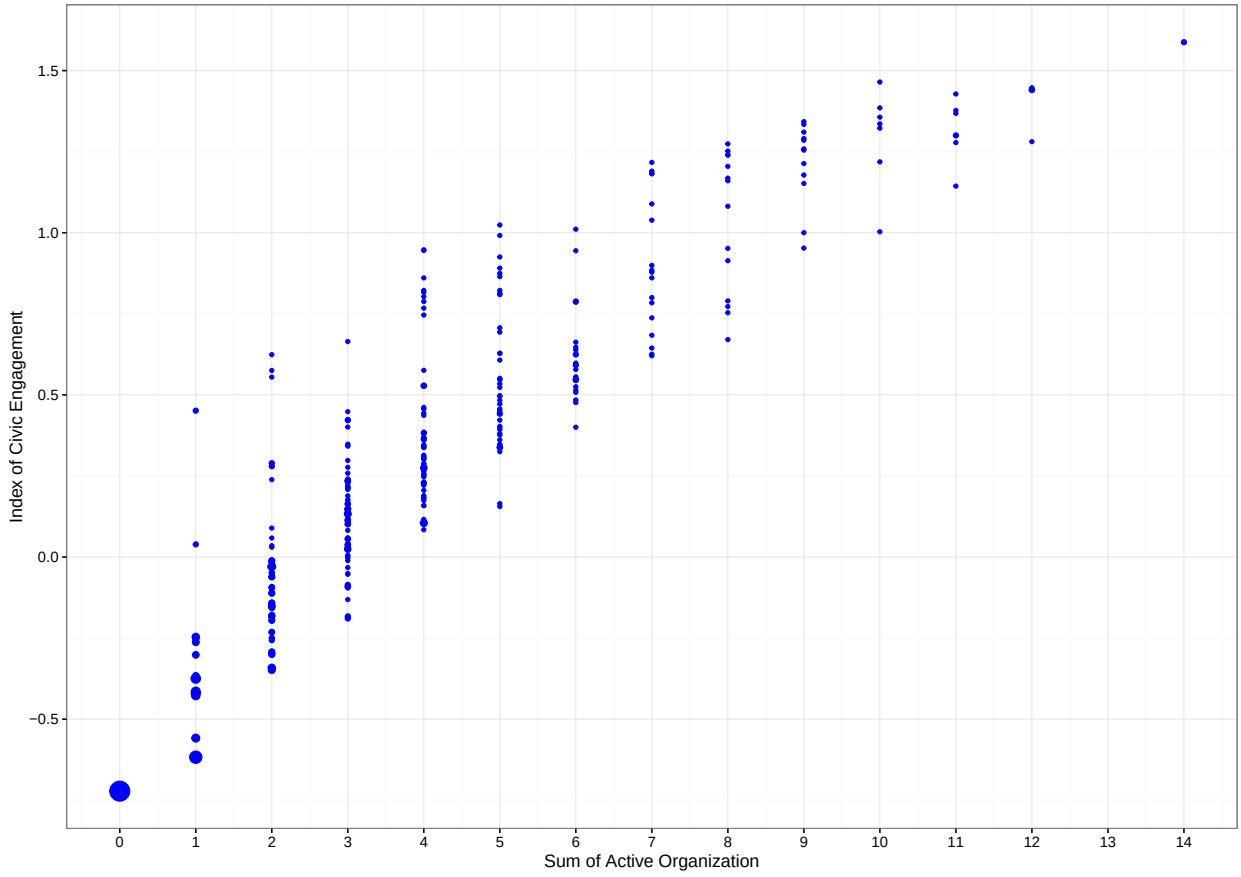
Table B.5: Number of respondents who lived in each province before 1975

Province Name	Number of Respondents (in province, before 1975)	Province Name	Number of Respondents (in province, before 1975)
An Giang	3	Kon Tum	0
Ba Ria	1	Lai Chau	21
Bac Giang	54	Lam Dong	9
Bac Kan	0	Lang Son	0
Bac Lieu	35	Lao Cai	15
Bac Ninh	1	Long An	0
Ben Tre	35	Nam Dinh	5
Binh Dinh	37	Nghe An	3
Binh Duong	0	Ninh Binh	2
Binh Phuoc	9	Ninh Thuan	1
Binh Thuan	1	Phu Tho	47
Ca Mau	2	Phu Yen	0
Can Tho	51	Quang Binh	3
Cao Bang	0	Quang Nam	38
Da Nang (City)	3	Quang Ngai	3
Dak Lak	19	Quang Ninh	2
Dong Nai	1	Quang Tri	1
Dong Thap	0	Soc Trang	4
Gia Lai	0	Son La	2
Ha Giang	0	Tay Ninh	3
Ha Nam	28	Thai Binh	48
Ha Noi (City)	67	Thai Nguyen	2
Ha Tay	3	Thanh Hoa	85
Ha Tinh	36	Thuathien-Hue	6
Hai Duong	3	Tien Giang	0
Hai Phong (City)	4	Tra Vinh	38
Ho Chi Minh (City)	104	Tuyen Quang	1
Hoa Binh	1	Vinh Long	4
Hung Yen	29	Vinh Phuc	2
Khanh Hoa	1	Yen Bai	2
Kien Giang	0		

Of the 63 total administrative units at the provincial level in Vietnam, 3 have no war data: Dak Nong, Dien Bien, and Hau Giang (excluded from the list above); and, 12 units do not have any respondent in the WVS dataset who lived in it before 1975.

B.6 Comparing the log factors to the raw items

Figure B.1: Comparison of the log of Civic Engagement Index to the N of Active Organizations



B.7 Results using Membership

Table B.6: The Impact of U.S. Bombings on Civic Engagement using Membership

	Civic Engagement Index (log)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.04^{***}	0.07^{***}	0.12^{***}	0.12^{***}
(Province, residence pre-1975)	(0.01)	(0.02)	(0.02)	(0.02)
Control Variables				
Education		0.02^{***}		0.02^{**}
		(0.01)		(0.01)
Gender		0.00		-0.01
		(0.02)		(0.02)
Age		-0.001		-0.001
		(0.001)		(0.001)
Population density (1960-61) ('000)		-0.01		-0.04^{**}
		(0.01)		(0.02)
Average precipitation ('00)		0.05		-0.09
		(0.07)		(0.07)
South		-0.25^{**}		-0.15
		(0.11)		(0.11)
Latitude ('0)		-0.15		0.01
		(0.10)		(0.10)
Intercept	0.52^{***}	0.74^{***}	0.46^{***}	0.50^{***}
	(0.02)	(0.20)	(0.04)	(0.20)
			First Stage	
Distance to the 17th Parallel			-0.42^{***}	-0.55^{***}
F Statistic (instrument)			483^{***}	354^{***}
Observations	875	862	875	862
Provinces	48	49	48	49
R^2	0.03	0.06		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the dependent variables to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.8 Results Excluding One Item Each Time

Table B.7: The Impact of U.S. Bombings on Civic Engagement Excluding One Item each Time

	Civic Engagement Index (log)			
	OLS	OLS	IV	IV
Excluded Item:	Model 1	Model 2	Model 3	Model 4
(1) Social welfare services	0.07***	0.11***	0.10***	0.21***
	(0.01)	(0.02)	(0.03)	(0.03)
(2) Religious organizations	0.08***	0.12***	0.10***	0.22***
	(0.01)	(0.02)	(0.03)	(0.03)
(3) Education, arts, music or cultural activities	0.07***	0.11***	0.11***	0.23***
	(0.01)	(0.02)	(0.03)	(0.04)
(4) Labor unions	0.09***	0.12***	0.12***	0.22***
	(0.01)	(0.02)	(0.03)	(0.04)
(5) Political groups or organizations	0.08***	0.11***	0.12***	0.20***
	(0.01)	(0.02)	(0.03)	(0.03)
(6) Local community action on social issues	0.07***	0.11***	0.10***	0.20***
	(0.01)	(0.02)	(0.03)	(0.03)
(7) Third world development or human rights	0.09***	0.12***	0.12***	0.24***
	(0.01)	(0.02)	(0.03)	(0.04)
(8) Conservation, environment, animal rights groups;	0.09***	0.12***	0.12***	0.23***
	(0.01)	(0.02)	(0.03)	(0.04)
(9) Professional associations	0.09***	0.13***	0.13***	0.25***
	(0.01)	(0.02)	(0.03)	(0.04)
(10) Youth work (e.g. scouts, guides, youth clubs etc.)	0.08***	0.12***	0.12***	0.24***
	(0.01)	(0.02)	(0.03)	(0.04)
(11) Sports or recreation	0.08***	0.11***	0.11***	0.22***
	(0.01)	(0.02)	(0.03)	(0.03)
(12) Women's groups	0.08***	0.12***	0.12***	0.23***
	(0.01)	(0.02)	(0.03)	(0.04)
(13) Peace movement	0.09***	0.12***	0.12***	0.25***
	(0.01)	(0.02)	(0.03)	(0.04)
(14) Voluntary organizations concerned with health	0.08***	0.12***	0.12***	0.24***
	(0.01)	(0.02)	(0.03)	(0.04)
Control Variables?	No	Yes	No	Yes
Observations	875	862	875	862
Provinces	48	49	48	49

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. The cells report the unstandardized coefficients of the effects of bombings on civic engagement. To estimate the log-log model, the value of 1 has been added to the dependent variables to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.9 Results using Birthplace

Table B.8: The Impact of Bombing on Civic Engagement (Birthplace)

	Civic Engagement Index (log)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.08^{***}	0.11^{***}	0.12^{***}	0.22^{***}
(Province, birthplace)	(0.01)	(0.02)	(0.02)	(0.03)
Control Variables				
Gender		-0.05		-0.06
		(0.03)		(0.03)
Age		-0.001		-0.001
		(0.001)		(0.001)
Education		0.03^{***}		0.03^{***}
		(0.01)		(0.01)
Population density (1960-61) ('000)		-0.05^{**}		-0.11^{***}
		(0.02)		(0.03)
Average precipitation ('00)		-0.06		-0.18^{*}
		(0.09)		(0.10)
South		-0.47^{***}		-0.32^{**}
		(0.14)		(0.14)
Latitude ('0)		-0.28^{**}		-0.02
		(0.13)		(0.14)
Intercept	-0.37^{***}	0.11	-0.47^{***}	0.25
	(0.03)	(0.26)	(0.06)	(0.27)
			First Stage	
Distance to the 17th Parallel			-0.44^{***}	-0.49^{***}
F Statistic (instrument)			566^{***}	837^{***}
Observations	986	970	986	970
Provinces	46	48	48	48
R^2	0.04	0.08		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.10 Results Excluding and Controlling for Members of Party and Trade Unions

B.10.1 Results Excluding Members of Parties and Trade Unions

Table B.9: The Impact of U.S. Bombings on Civic Engagement excluding Members of Parties and Trade Unions

	Civic Engagement Index (log)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.05^{***}	0.06^{***}	0.07^{***}	0.13^{***}
(Province, residence pre-1975)	(0.01)	(0.02)	(0.02)	(0.03)
Control Variables				
Education		0.00		-0.01
		(0.01)		(0.01)
Gender		-0.06^{***}		-0.06^{*}
		(0.03)		(0.03)
Age		-0.003^{*}		-0.003^{***}
		(0.002)		(0.001)
Population density (1960-61) ('000)		0.00		-0.04
		(0.02)		(0.03)
Average precipitation ('00)		0.08		-0.04
		(0.09)		(0.10)
South		-0.28^{**}		-0.23^{*}
		(0.14)		(0.14)
Latitude ('0)		-0.18		-0.05
		(0.12)		(0.13)
Intercept	-0.48^{***}	-0.01	-0.53^{***}	-0.17^{***}
	(0.02)	(0.26)	(0.05)	(0.25)
Observations	586	569	586	569
Provinces	44	43	44	43
R^2	0.03	0.05		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the dependent variables to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.10.2 Results Controlling for Party and Trade Union Membership

Table B.10: The Impact of U.S. Bombings on Civic Engagement Controlling for Party and Trade Union Membership

	Civic Engagement Index (log)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.06^{***}	0.07^{***}	0.08^{***}	0.14^{***}
(Province, residence pre-1975)	(0.01)	(0.02)	(0.02)	(0.03)
Control Variables				
Party Membership	0.49^{***}	0.48^{***}	0.48^{***}	0.46^{***}
	(0.04)	(0.04)	(0.04)	(0.04)
Trade Union Membership	0.35^{***}	0.35^{***}	0.34^{***}	0.35^{***}
	(0.06)	(0.06)	(0.06)	(0.06)
Education		-0.01		-0.01
		(0.01)		(0.01)
Gender		-0.03		-0.03
		(0.03)		(0.03)
Age		-0.002		-0.002
		(0.001)		(0.001)
Population density (1960-61) ('000)		-0.03		-0.06^{**}
		(0.02)		(0.02)
Average precipitation ('00)		0.08		-0.09
		(0.08)		(0.10)
South		-0.23^{*}		-0.14
		(0.12)		(0.14)
Latitude ('0)		-0.18^{*}		-0.02
		(0.11)		(0.13)
Intercept	-0.50^{***}	-0.53^{***}	-0.57^{***}	-0.26
	(0.03)	(0.24)	(0.06)	(0.28)
Observations	875	851	875	851
Provinces	49	48	49	48
R^2	0.31	0.32		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the dependent variables to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.11 Results Excluding Sub-samples of Observations

B.11.1 Results Excluding Quang Tri province

Table B.11: Results excluding Quang Tri province (the most heavily bombed province)

	Civic Engagement Index (log)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.08^{***}	0.12^{***}	0.12^{***}	0.24^{***}
	(0.01)	(0.02)	(0.03)	(0.03)
Control Variables				
Gender		-0.06^{*}		-0.07^{*}
		(0.04)		(0.04)
Age		-0.001		-0.002
		(0.001)		(0.001)
Education		0.02^{**}		0.02^{*}
		(0.01)		(0.01)
Population density (1960-61) ('000)		-0.06^{**}		-0.11^{***}
		(0.02)		(0.03)
Average Precipitation ('00)		0.02		-0.22^{**}
		(0.10)		(0.11)
South		-0.49^{***}		-0.31^{*}
		(0.16)		(0.16)
Latitude ('0)		-0.31^{**}		0.001
		(0.15)		(0.02)
Intercept	-0.39^{***}	0.29	-0.47^{***}	-0.02
	(0.03)	(0.31)	(0.07)	(0.16)
F Statistic (instrument)			442^{***}	608^{***}
Observations	874	861	874	861
R^2	0.04	0.07		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.11.2 Results Excluding the Most Heavily Bombed Observations (top 20%)

Table B.12: Results Excluding the Top 20% Most Heavily Bombed Observations

	Civic Engagement Index (log)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.10^{***}	0.12^{***}	0.16^{***}	0.22^{***}
	(0.01)	(0.02)	(0.04)	(0.03)
Control Variables				
Gender		-0.06		-0.07^{*}
		(0.04)		(0.04)
Age		-0.001		-0.001
		(0.001)		(0.001)
Education		0.02^{*}		0.02
		(0.01)		(0.01)
Population density (1960-61) ('000)		-0.13^{**}		-0.24^{***}
		(0.06)		(0.03)
Average Precipitation ('00)		0.02		-0.13
		(0.13)		(0.11)
South		-0.58^{**}		-0.37^{*}
		(0.17)		(0.16)
Latitude ('0)		-0.04^{*}		-0.04
		(0.02)		(0.16)
Intercept	-0.40^{***}	0.40	-0.54^{***}	-0.21
	(0.03)	(0.42)	(0.08)	(0.33)
F Statistic (instrument)			241^{***}	580^{***}
Observations	705	692	705	692
R^2	0.04	0.08		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.11.3 Results Excluding Provinces with less than 5 respondents

Table B.13: Results Excluding Provinces with less than 5 respondents

	Civic Engagement Index (log)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.08^{***}	0.11^{***}	0.10^{***}	0.22^{***}
	(0.01)	(0.02)	(0.03)	(0.03)
Control Variables				
Gender		-0.06		-0.07^{*}
		(0.04)		(0.04)
Age		-0.002		-0.002
		(0.001)		(0.001)
Education		0.02^{*}		0.02^{*}
		(0.01)		(0.01)
Population density (1960-61) ('000)		-0.05^{*}		-0.10^{***}
		(0.02)		(0.03)
Average Precipitation ('00)		0.02		-0.22
		(0.10)		(0.11)
South		-0.49^{**}		-0.32^{*}
		(0.16)		(0.16)
Latitude ('0)		-0.32		-0.04
		(0.15)		(0.16)
Intercept	-0.37^{***}	0.34	-0.43^{***}	-0.08
	(0.03)	(0.31)	(0.07)	(0.32)
F Statistic (instrument)			392^{***}	557^{***}
Observations	816	804	816	804
R^2	0.03	0.08		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.12 Robustness to only North Vietnam

Table B.14: The Effect of Bombing on Engagement in North Vietnam

	Civic Engagement Index (log)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.15^{***}	0.16^{***}	0.21^{***}	0.21^{***}
	(0.02)	(0.02)	(0.03)	(0.02)
Control Variables				
Gender		0.02		-0.02
		(0.05)		(0.05)
Age		0.0003		0.0002
		(0.002)		(0.002)
Education		0.02		0.01
		(0.01)		(0.01)
Population density (1960-61) ('000)		-0.17^{***}		-0.22^{***}
		(0.06)		(0.06)
Average precipitation		0.03		-0.11
		(0.15)		(0.15)
Intercept	-0.45^{***}	-0.53^{**}	-0.57^{***}	-0.37
	(0.03)	(0.25)	(0.05)	(0.25)
			First Stage	
Distance to the 17th Parallel			-0.86^{***}	-1.10^{***}
			(0.03)	(0.03)
Controls?			N	Y
F Statistic (instrument)			773^{***}	1532^{***}
Observations	473	464	473	464
R^2	0.13	0.15		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.13 Robustness to Non-random Attrition: Deaths and International Migrants

Estimates of the number of Vietnamese casualties due to the Vietnam War vary widely. Lewy (1978) estimated that $1,353,000$ military and civilian Vietnamese died during the 1965–1975 period. Based on longitudinal comparisons of life tables, Hirschman, Preston and Loi (1995) published that the most likely estimate of the number of deaths in the Vietnam War was $882,000$, an estimate within the confidence interval of $791,000$ and $1,141,000$. The Vietnamese government increased the estimate to 3 million military and civilian deaths (Shenon, 1995). Most recently, Obermeyer, Murray and Gakidou (2008) estimated that the most likely death toll caused directly by the Vietnam War during the 1955–1975 period, or indirectly in the post-conflict 1975–2002 period was 3.8 million people.

I take this latest estimate as the most liberal estimate of Vietnamese deaths. Adding to the number of deaths, a number of Vietnamese fled the country during or after the war. By 1995, the estimated number of Vietnamese migrants was of $480,000$ in the United States, $210,000$ in other countries, and $46,000$ still lived in refugee camps in other Asian countries. This accounts for 736,000 international migrants. The population of Vietnam was $78,620,500$ in 2001 (Nations, 2013). Therefore, the most likely attrition rate due to the Vietnam War is 5.8% of the 2001 total population. However, I check my findings to an implausibly large attrition rate of 11% (about double the size of the most likely attrition rate). To do this, I generate artificially attrited observations equivalent to a 11% of the survey sample through a weighted re-sampling with replacement from the observed respondents. The weights are based on the value of bombings intensity. Thus, the likelihood that an observation from the original sample is re-used as an artificial observation in the final sample is proportional to

the intensity of the conflict in the province of residence before 1975. Finally, I assign the lowest possible value to the civic engagement to these re-sampled observations.

Table B.15 reports the results from the re-estimation based on extreme outcomes. It shows that the main finding would remain statistically and substantively significant even in the worst-case scenario of attrition from deaths and international migrants.

Table B.15: Robustness to Non-random Attrition: Deaths and International Migrants

	Civic Engagement Index (log)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.05^{***}	0.10^{***}	0.07^{***}	0.21^{***}
	(0.01)	(0.02)	(0.03)	(0.03)
Control Variables				
Gender		-0.04		-0.04
		(0.03)		(0.03)
Age		-0.002		-0.002
		(0.001)		(0.001)
Education		0.02^{**}		0.02^{*}
		(0.01)		(0.01)
Population density (1960-61) ('000)		-0.06^{***}		-0.11^{***}
		(0.02)		(0.03)
Average precipitation ('00)		-0.09		-0.34^{***}
		(0.09)		(0.10)
South dummy		-0.38^{**}		-0.18
		(0.15)		(0.16)
Latitude ('0)		-0.21		0.07
		(0.14)		(0.14)
Intercept	-0.34^{***}	0.24	-0.39^{***}	-0.26
	(0.03)	(0.28)	(0.07)	(0.32)
F Statistic (instrument)			514^{***}	658^{***}
Observations	957	957	957	957
R^2	0.01	0.06		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.14 Sensitivity Analysis to Violations of the Exclusion Restriction

In a generalization of the IV regression that allows the instrument to enter linearly in the structural equation, we can define a parameter δ that reflects how close the exclusion restriction is to being satisfied in a model like the following: $Y = \beta X + \delta Z + \epsilon$, where Y is the outcome, X is the endogenous variable, ϵ are unobservables, and Z is the instrumental variable that is assumed uncorrelated with ϵ, which is the basis of the estimation. The exclusion restriction is, thus, equivalent to a prior and strict belief that $\delta = 0$. Using these equations, we can see how much change in the estimates result from violations in the exclusion restriction (Conley, Hansen and Rossi, 2012) by changing the values in δ.

Table B.16 shows that the main finding is robust to a violation of the exclusion restriction equivalent to $\delta = 0.19$ (90% CI). This magnitude of δ should be compared to the standardized coefficients of the reduced-form equation (not shown in Table), which is 0.41 (see main text for further information).

Table B.16: Sensitivity Analysis: The Treatment Effect of Bombing on Civic Engagement under Violations of the Exclusion Restriction

	Civic Engagement Index (log)	
	$\hat{\beta}$	90% CI
	0.23	[0.18 0.29]
δ		
$\delta \in (-0.01, 0.01)$		[0.17 0.29]
$\delta \in (-0.02, 0.02)$		[0.17 0.30]
$\delta \in (-0.03, 0.03)$		[0.16 0.31]
$\delta \in (-0.04, 0.04)$		[0.15 0.32]
$\delta \in (-0.05, 0.05)$		[0.14 0.33]
$\delta \in (-0.06, 0.06)$		[0.12 0.34]
$\delta \in (-0.07, 0.07)$		[0.12 0.35]
$\delta \in (-0.08, 0.08)$		[0.11 0.36]
$\delta \in (-0.09, 0.09)$		[0.10 0.37]
$\delta \in (-0.10, 0.10)$		[0.09 0.38]
$\delta \in (-0.11, 0.11)$		[0.09 0.39]
...		...
$\delta \in (-0.19, 0.19)$		[−0.00 0.49]

Note: $\hat{\beta}$ refer to the unstandardized coefficients.

B.15 Plots of Predicted Values with Natural Scales for Full Distribution

Figure B.2: Predicted Values of Civic Engagement at Different Levels of Bombing Intensity (Natural Scales with Full Distribution of Bombing Intensity)

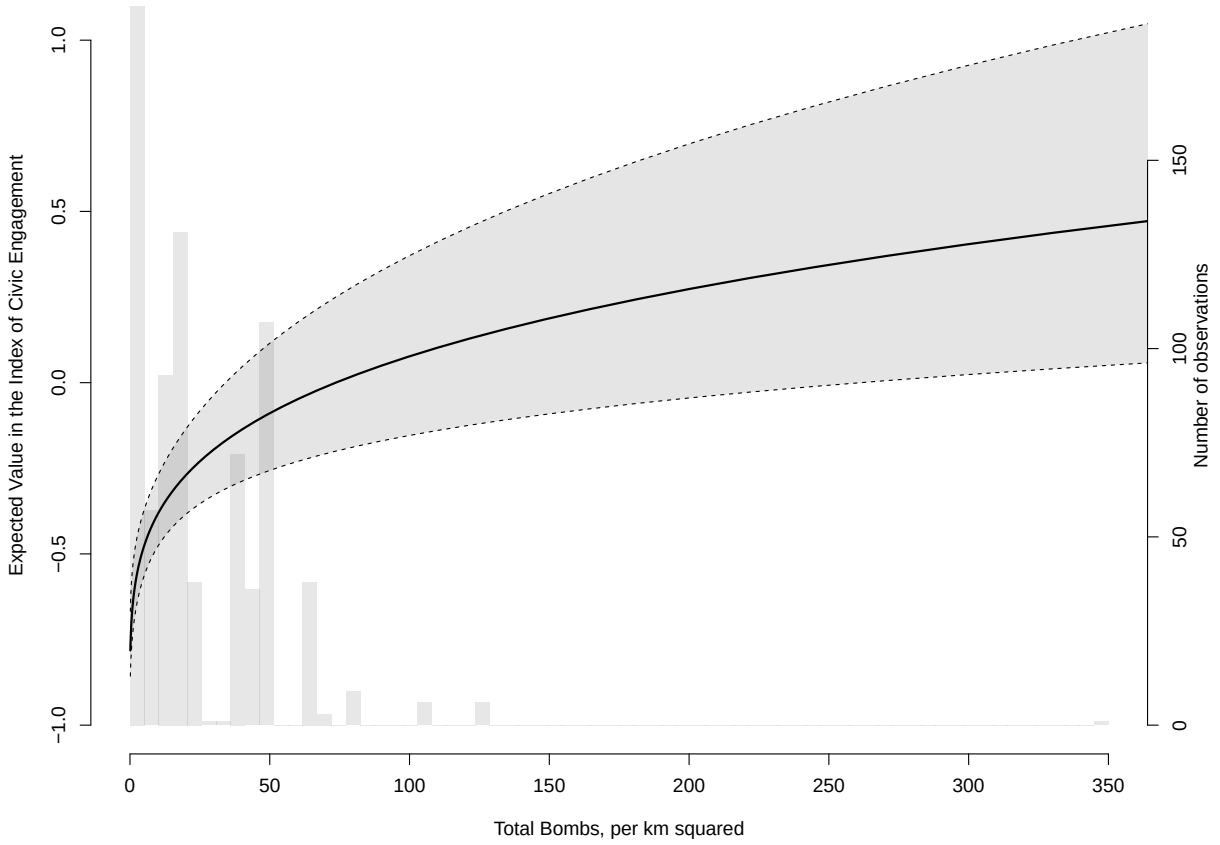


Figure B.3: Natural Scales

Predicted values are generated from Model 4 in Table 2.1, and keeping all continuous control variables at their mean value, and categorical control variables at their median value.

B.16 Results for the Participatory Values Index. Full Models for the Index and the Individual Items

B.16.1 Summary Models for Individual Items

Table B.17: The Impact of U.S. Bombing on Participatory Values

	DV: Participatory Values (Item 1)			
	OLS	OLS	IV	IV
PANEL A: Participatory Values (Item 1)	Model 1	Model 2	Model 3	Model 4
Key Independent Variable:				
Total Bombs, per km^2 (log)	0.03**	0.05***	0.05***	0.10***
	(0.01)	(0.01)	(0.01)	(0.02)
	DV: Participatory Values (Item 2)			
	OLS	OLS	IV	IV
PANEL B: Participatory Values (Item 2)	Model 1	Model 2	Model 3	Model 4
Key Independent Variable:				
Total Bombs, per km^2 (log)	0.02**	0.05**	0.02	0.07**
	(0.01)	(0.01)	(0.02)	(0.02)
Controls?	N	Y	N	Y
Observations	875	862	875	862
Provinces	48	47	48	47

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province. See full model in Tables B.19 and B.20.

B.16.2 Full Model from Table 2.3 (Panel A)

Table B.18: The Effect of Bombing on the Index of Participatory Values

	DV: Participatory Values (Index			
	OLS Model 1	OLS Model 2	IV Model 3	IV Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.01**	0.04***	0.03***	0.06**
	(0.01)	(0.01)	(0.01)	(0.02)
Control Variables				
Gender		−0.01		−0.01
		(0.01)		(0.02)
Age		−0.001*		−0.001*
		(0.001)		(0.001)
Education		0.01*		0.01*
		(0.01)		(0.01)
Population density (1960-61) ('000)		−0.05***		−0.06***
		(0.01)		(0.01)
Average precipitation ('00)		−0.10**		−0.18***
		(0.04)		(0.05)
South		0.04		0.10
		(0.07)		(0.08)
Latitude ('0)		0.10		0.18**
		(0.07)		(0.08)
Intercept	0.26***	0.19	0.21***	0.06
	(0.02)	(0.14)	(0.03)	(0.16)
Observations	875	862	875	862
R^2	0.04	0.04		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.16.3 Full Model of Item 1 of the Index of Participatory Values

Table B.19: The Effect of Bombing on the Index of Participatory Values (item 1)

	DV: Participatory Values (item 1)			
	OLS Model 1	OLS Model 2	IV Model 3	IV Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.03**	0.05***	0.05***	0.10***
	(0.01)	(0.01)	(0.01)	(0.02)
Control Variables				
Gender		−0.02		−0.02
		(0.02)		(0.02)
Age		−0.002		−0.002**
		(0.001)		(0.001)
Education		0.01		0.01
		(0.01)		(0.01)
Population density (1960-61) ('000)		−0.04***		−0.06***
		(0.01)		(0.02)
Average precipitation ('00)		−0.05		−0.16***
		(0.05)		(0.05)
South		−0.04		0.04
		(0.07)		(0.08)
Latitude ('0)		0.03		0.16**
		(0.07)		(0.08)
Constant	0.30***	0.35**	0.23***	0.15
	(0.02)	(0.15)	(0.03)	(0.16)
Observations	875	862	875	862
R^2	0.01	0.04		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.16.4 Full Model of Item 2 of the Index of Participatory Values

Table B.20: The Effect of Bombing on the Index of Participatory Values (item 2)

	DV: Participatory Values (item 2)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
Key Independent Variable				
Total Bombs, per km^2 (log)	0.02^{**}	0.05^{**}	0.02	0.07^{**}
	(0.01)	(0.01)	(0.02)	(0.02)
Control Variables				
Gender		0.01		0.01
		(0.02)		(0.02)
Age		−0.001		−0.001
		(0.001)		(0.001)
Education		0.01^{**}		0.01^{**}
		(0.01)		(0.01)
Population density (1960-61) ('000)		-0.05^{***}		-0.06^{***}
		(0.01)		(0.02)
Average precipitation ('00)		-0.16^{**}		-0.27^{***}
		(0.06)		(0.21)
South		0.10		0.14
		(0.10)		(0.11)
Latitude ('0)		0.14^{*}		0.02^{*}
		(0.09)		(0.01)
Intercept	0.16^{***}	0.02	0.14^{***}	−0.07
	(0.02)	(0.20)	(0.04)	(0.21)
Observations	875	862	875	862
R^2	0.01	0.03		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.17 Alternative Mechanisms

B.17.1 Post-conflict Public Investments

Table B.21: The Effect of the Post-conflict Public Investments

	DV: Civic Engagement (log)	
	OLS	OLS
	Model 1	Model 2
Key Alternative Variable		
Σ State Investments per capita, 1976-1985	-0.01	-0.01
	(0.02)	(0.03)
Control Variables		
Gender		-0.07^{*}
		(0.04)
Age		-0.002
		(0.001)
Education		0.03^{**}
		(0.01)
Population density (1960-61) ('000)		0.01
		(0.04)
Average precipitation ('00)		0.06
		(0.10)
South		-0.42
		(0.16)
Latitude ('0)		-0.21
		(0.15)
Intercept	-0.16^{***}	0.02
	(0.02)	(0.33)
Observations	845	832
Provinces	46	45

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

B.17.2 Historical Versus Contemporaneous Effects

Full Model from Table 2.4, Panel A

Table B.22: The Effect of Post-Conflict Province-Level Change in Population on Civic Engagement. Full Models

	DV: Civic Engagement (log)	
	OLS	OLS
	Model 1	Model 2
Key Alternative Variable		
Δ Population: 1985-1976	0.14^{*}	0.14
	(0.08)	(0.09)
Control Variables		
Gender		-0.04
		(0.04)
Age		-0.001
		(0.001)
Education		0.03^{**}
		(0.01)
Population density (1960-61) ('000)		0.01
		(0.02)
Average precipitation ('00)		0.24^{*}
		(0.14)
South		-0.63^{**}
		(0.26)
Latitude ('0)		-0.06^{**}
		(0.02)
Intercept	-0.21^{***}	0.70^{*}
	(0.02)	(0.40)
Observations	764	751
Provinces	32	31

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

Full Model from Table 2.4, Panel B

Table B.23: The Differential Effect of Bombing in the 2001 Province of Residence Compared to the pre-1975 Province of Residence. Full Models

	DV: Civic Engagement (log)			
	OLS	OLS	IV	IV
	Model 1	Model 2	Model 3	Model 4
Key Independent Variable				
Total Bombs, per km^2 (log) pre-1975	0.08^{***}	0.12^{***}	0.12^{***}	0.25^{***}
	(0.01)	(0.02)	(0.03)	(0.04)
Key Alternative Variable				
Total Bombs, per km^2 (log) 2001	0.003	0.04	0.03	0.10^{**}
— Total Bombs, per km^2 (log) pre-1975	(0.04)	(0.05)	(0.03)	(0.05)
Control Variables				
Age		−0.002		−0.002
		(0.001)		(0.001)
Gender		-0.06^{*}		−0.07
		(0.04)		(0.04)
Education		0.03^{**}		0.02^{*}
		(0.01)		(0.01)
Pop. density (1960-61) pre-1975 ('000)		-0.06^{**}		-0.12^{***}
		(0.02)		(0.03)
Pop. density (1960-61) 2001 ('000)		−0.01		−0.05
— Pop. density (1960-61) pre-1975 ('000)		(0.06)		(0.05)
Average precipitation pre-1975 ('00)		0.05		−0.20
		(0.11)		(0.12)
Average precipitation 2001 ('00)		0.16		0.06
— Average precipitation pre-1975 ('00)		(0.23)		(0.24)
South pre-1975		-0.47^{***}		−0.30
		(0.18)		(0.19)
South 2001 — South pre-1975		0.12		0.15
		(0.34)		(0.35)
Latitude pre-1975 ('0)		-0.29^{*}		−0.02
		(0.16)		(0.18)
Latitude 2001 ('0) — Latitude pre-1975 ('0)		0.06		0.12
		(0.34)		(0.35)
Intercept	-0.38^{***}	0.21	-0.47^{***}	−0.21
	(0.03)	(0.32)	(0.07)	(0.34)
Difference in coefficients, p-value	0.02	0.06	< 0.01	< 0.01
Observations	875	862	875	862
Provinces	49	48	49	48
R^2	0.04	0.09		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province. The *difference in coefficients* refers to the difference between the effect of the key independent variable, the exposure to bombings in the pre-1975 period, and the key alternative variable, the difference in exposure to bombings in the post-1975 and the pre-1975 period.

Alternative Specifications

Alternative specifications to deal with post-conflict development using respondents' tracking information on their distinct places of residence involve either restricting the sample to only those respondents who report a different province of residence in the 1990-2001 period with respect to the pre-1975, or including simultaneously the value of bombings in the province of residence in the pre-1975 period and the value of bombing in the 1990-2001 period in the same regression. Both are ineffective and inefficient modelling strategies.

On the one hand, the migrant-only strategy involves working with a sample of only 123 respondents, which increases the number of Type II errors and lead to biased inferences in the IV models given that the consistency of the IV estimator is only a large sample property. On the other hand, regressing the dependent variable on both variables, bombings in the pre-1975 province of residence and bombings in the 1990-2001 province of residence is problematic due to extreme multicollinearity, $r = 0.90$, of the two variables. Thus, it cannot reliably identify their unique effect.

Notwithstanding this, Table B.24 reports a summary of the results from these two alternative specifications. The substantive conclusions remain unchanged; that is, we cannot detect a consistently significant effect of a respondents' post-conflict context once we take into account a respondents' conflict exposure.

Table B.24: The Differential Effect of Bombing in the 2001 Province Compared to the pre-1975 Province (Alternative Specification)

	DV: Civic Engagement (log)			
PANEL A: Entire Sample	OLS Model A.1	OLS Model A.2	IV Model A.3	IV Model A.4
Total Bombs, per km^2 (log): pre-1975	0.08**	0.08**	0.23**	0.38***
	(0.03)	(0.04)	(0.11)	(0.08)
Total Bombs, per km^2 (log): 2001	0.00	0.05	−0.14	−0.18**
	(0.03)	(0.04)	(0.10)	(0.07)
Difference in coefficients, p-value	0.24	0.64	0.05	< 0.01
Controls?	N	Y	N	Y
Observations	875	862	875	862
Provinces	49	48	49	48
R^2	0.04	0.09		
PANEL B: Migrants only	OLS Model B.1	OLS Model B.2	IV Model B.3	IV Model B.4
Total Bombs, per km^2 (log): pre-1975	0.10**	0.15**	0.16**	0.30**
	(0.04)	(0.07)	(0.08)	(0.11)
Key Alternative Variable				
Total Bombs, per km^2 (log): 2001	0.07	0.08	0.05	0.11*
	(0.05)	(0.04)	(0.05)	(0.05)
Difference in coefficients, p-value	0.41	0.43	0.14	0.09
Controls?	N	Y	N	Y
Observations	123	122	123	122
Provinces	43	42	43	42
R^2	0.04	0.08		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Individual controls: age, gender, and education. Geographic controls: log of the population density of the province in 1960, average precipitation, average temperature, south dummy, latitude. Each geographic controls includes two variables in the model: its values in the pre-1975 province, and its values in 2001 minus its values in pre-1975. Instrumental Variable: Distance to the parallel 17, pre-1975. Constants and control variables are omitted from the output. All models include heteroskedastic clustered-standard errors at the province level in parenthesis. For the full models, see Tables B.25 and B.26 below. The *difference in coefficients* refers to the difference between the effect of the key independent variable, the exposure to bombings in the pre-1975 period, and the key alternative variable, the difference in exposure to bombings in the post-1975 and the pre-1975 period.

Table B.25: The Differential Effect of Bombing in the 2001 province of residence compared to the pre-1975 province of residence (Entire Sample)

	DV: Civic Engagement (log)			
	OLS Model 1	OLS Model 2	IV Model 3	IV Model 4
Key Independent Variable				
Total Bombs, per km^2 (log): pre-1975	0.08^{**}	0.08^{**}	0.23^{**}	0.38^{***}
	(0.03)	(0.04)	(0.11)	(0.08)
Key Alternative Variable				
Total Bombs, per km^2 (log): 2001	0.00	0.05	-0.14	-0.18^{**}
	(0.03)	(0.04)	(0.10)	(0.07)
Control Variables				
Gender		-0.06^{*}		-0.06
		(0.04)		(0.04)
Age		-0.002		-0.002
		(0.001)		(0.001)
Education		0.03^{**}		0.02^{*}
		(0.01)		(0.01)
Population density (1960-61) ('000)		-0.06^{***}		-0.10^{***}
				(0.03)
Average precipitation ('00)		0.02		-0.26^{**}
		(0.10)		(0.11)
South		-0.48^{**}		-0.19
		(0.16)		(0.17)
Latitude ('0)		-0.30^{**}		0.04
		(0.15)		(0.17)
Intercept	-0.38^{***}	0.26	-0.42^{***}	-0.19
	(0.03)	(0.32)	(0.04)	(0.33)
Difference in coefficients, p-value	0.24	0.64	0.05	< 0.01
Observations	875	862	875	862
Provinces	49	48	49	48
R^2	0.04	0.09		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province. The *difference in coefficients* refers to the difference between the effect of the key independent variable, the exposure to bombings in the pre-1975 period, and the key alternative variable, the difference in exposure to bombings in the post-1975 and the pre-1975 period for the entire sample.

Table B.26: The Differential Effect of Bombing in the 2001 province of residence compared to the pre-1975 province of residence (Migrant-only Sample)

	DV: Civic Engagement (log)			
	OLS Model 1	OLS Model 2	IV Model 3	IV Model 4
Key Independent Variable				
Total Bombs, per km^2 (log): pre-1975	0.10^{**}	0.15^{**}	0.16^{**}	0.30^{**}
	(0.04)	(0.07)	(0.08)	(0.11)
Key Alternative Variable				
Total Bombs, per km^2 (log): 2001	0.04	0.08	0.03	0.08
	(0.04)	(0.05)	(0.05)	(0.05)
Control Variables				
Gender		−0.10		−0.07
		(0.12)		(0.12)
Age		−0.005		−0.005
		(0.005)		(0.005)
Education		0.02		0.01
		(0.03)		(0.03)
Population density (1960-61) ('000)		−0.06		-0.12^{*}
		(0.09)		(0.09)
Average precipitation ('00)		−0.15		−0.49
		(0.26)		(0.31)
South		−0.10		0.18
		(0.43)		(0.46)
Latitude ('0)		0.17		0.57
		(0.41)		(0.47)
Intercept	-0.49^{***}	−0.58	-0.61^{***}	−1.21
	(0.14)	(1.14)	(0.18)	(1.22)
Difference in coefficients, p-value	0.41	0.43	0.14	0.09
Observations	123	122	123	122
Provinces	44	43	44	43
R^2	0.07	0.08		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province. The *difference in coefficients* refers to the difference between the effect of the key independent variable, the exposure to bombings in the pre-1975 period, and the key alternative variable, the difference in exposure to bombings in the post-1975 and the pre-1975 period for those who migrated at the end of the war.

B.17.3 Personal Grievances

Measurement

I construct two measures to adjust the main coefficients of the long-term effects of bombings on civic engagement for personal grievances. Because war exposure may have caused individual difficulties such as worse psychological well-being or physical impairment, it might be the case that these constitute the pathways of the relationship found in the main analyses. Though I do not include these in the main models because they are likely to be post-treatment, I do re-run the analysis with them to check whether there is suggestive evidence for these alternative pathways.

First, I measure self-reported happiness with the following question included in the World Value Survey: "Taking all things together, would you say you are: very happy (4), quite happy (3), not very happy (2), not at all happy (1), don't know (NA)". Second, I measure self-reported health with the following question included in the World Value Survey: "All in all, how would you describe your state of health these days? Would you say it is... very good (4), good (3), fair (2), poor (1), don't know (NA)".

Full Models

Table B.27 reports the results of the main specification after adjusting for the two variables capturing personal grievances: self-reported happiness and health. First, neither of the two variable are significantly related to civic engagement. Furthermore, the main result remains unaltered after adjusting for personal grievances. Taken together, the evidence provided here suggests that it is unlikely that either happiness or health play any large role in driving the persistent effects of exposure to war-related violence on civic engagement.

Table B.27: The Effect of Bombing after adjusting for Personal Grievances

	DV: Civic Engagement (log)			
	OLS Model 1	OLS Model 2	IV Model 3	IV Model 4
Key Independent Variable				
Total Bombs, per km^2 (log): pre-1975	0.08^{***}	0.12^{***}	0.11^{***}	0.22^{***}
	(0.01)	(0.02)	(0.03)	(0.03)
Control Variables				
Happiness	0.03	0.03	0.02	0.03
	(0.03)	(0.03)	(0.03)	(0.03)
Self-reported health	−0.01	−0.03	−0.01	−0.03
	(0.02)	(0.02)	(0.02)	(0.02)
Gender		-0.07^{*}		−0.08
		(0.04)		(0.04)
Age		−0.002		−0.003
		(0.001)		(0.001)
Education		0.03^{**}		0.02^{*}
		(0.01)		(0.01)
Population density (1960-61) ('000)		-0.05^{**}		-0.10^{***}
		(0.02)		(0.03)
Average precipitation ('00)		0.002		-0.22^{**}
		(0.09)		(0.11)
South dummy		-0.45^{***}		-0.29^{*}
		(0.16)		(0.17)
Latitude ('0)		-0.28^{*}		0.03
		(0.15)		(0.16)
Intercept	-0.43^{***}	0.26	-0.49^{***}	0.10
	(0.07)	(0.33)	(0.07)	(0.31)
Observations	868	857	868	857
Provinces	49	48	49	48
R^2	0.04	0.07		

Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. To estimate the log-log model, the value of 1 has been added to the outcome to ensure it has positive values throughout its range. All models have robust clustered-standard errors at the level of the province.

Appendix C

Mobilizing Collective Memory: A Field Experiment on the Effects of Collective Threat on Voting Behavior

C.1 Ethical Considerations

Several ethical considerations of the field experiment merit mention. This experiment reports the results of a clustered randomized field experiment conducted 2-3 days before the 2017 Catalan regional elections. I followed three ethical principles in the implementation of the field experiment:

1. *No interference.* My intervention did not change reality as the NGO's campaign would have been implemented in any scenario. My contribution to the campaign lies in the random assignment of treatment conditions and the replacement of some treatment leaflet for placebo leaflets.

2. *Not too sensitive.* The image used in the experiment had been popular in the regular media in the weeks after October 1, 2017. The image included no explicit violence (e.g., no blood) or vulnerable groups (e.g., children or elderly).

3. *No electoral impact. Ex ante* size of the sample insufficient to directly influence electoral outcomes (i.e., number of seats in the Parliament). The upper bound of the treated population amounts to 37,195 (inhabitants in treated villages who live within the urban nucleus), which is equivalent to $<0.1\%$ of total voters. Consequently, the number of people was clearly insufficient to impact the electoral results.

C.2 Sample and Population Descriptive Statistics

Table C.1 reports the differences between the descriptive statistics of the villages in the sample and in the rest of the population. This exercise allows us to evaluate the representatives of the sample compared to the entire population of villages. The first column reports the mean difference between those villages in the sample and those that are not conditional on all other covariates in the Table. For this, I regress each variable on a dummy that indicates whether the villages is included in the field experiment and all other covariates. Column 1 reports the coefficient of this dummy indicator.

The sample in the field experiment is not substantively different from the rest of the population of villages across most covariates. Those villages that are in the sample of the field experiment turned out, on average, 1.07 percentage points less than those that are not in the sample. Even though statistically significant, the magnitude of the difference does not seem large. For the rest of variables, we see that, on average, those in the sample are 3.6 kilometres closer to their province capital, voted 1.27 percentage points less for pro-independence parties in the 2015 regional elections, have a difference in the number of eligible voters that is less than one voter, their share of Catalan-born population is almost identical, and have more villages from the province of Barcelona and less from Girona and Lleida. However, these differences are not substantively or statistically significant.

In conclusion, we can see no obvious differences between the sample descriptive statistics and the population descriptive statistics across observable variables. Consequently, the sample average treatment effect obtained from the field experiment is likely to be similar to the population average treatment effect. However, there is always the chances that these two types of villages are different in terms of unobservable covariates. Yet, it is hard to imagine what unobservable characteristic could be sufficiently relevant as to alter the main results.

Table C.1: Sample and Population Descriptive Statistics

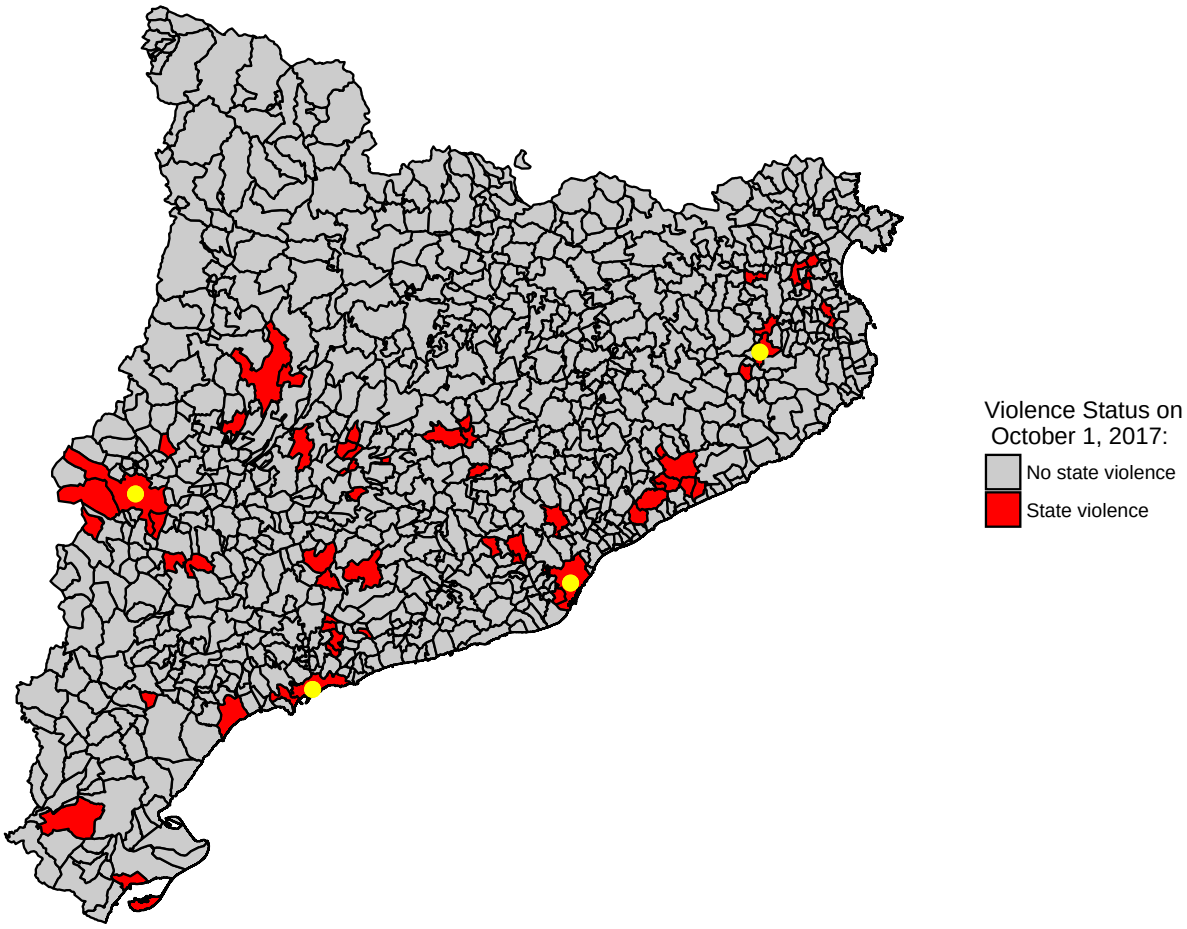
	Conditional Mean Difference: In Sample − Out Sample (N = 140) (N = 518)	t-value (p-value)
Distance to Province Capital (meters)	−3,671.9 (7,354.6)	−0.54 (0.62)
Turnout rate 2015	−1.07 (0.40)	−2.66 (0.01)
Vote for pro-Independence Parties 2015	−1.27 (0.97)	−1.32 (0.18)
Number of Eligible Voters 2015	0.52 (46.6)	0.01 (0.99)
Population density	5.08 (16.2)	0.31 (0.75)
Share of Catalan-born Population	0.00 (0.01)	0.26 (0.79)
Girona Province Population	−0.03 (0.04)	−0.76 (0.45)
Barcelona Province Population	0.06 (0.04)	1.43 (0.15)
Tarragona Province Population	0.01 (0.04)	0.17 (0.87)
Lleida Province Population	−0.03 (0.05)	−0.72 (0.47)

Note: This table reports the conditional mean difference between villages included and excluded from the sample using publicly available municipality level data. Column (1) presents the conditional mean difference (and standard error in parenthesis). Columns (2) report the t-statistic of this difference and its associated p-value. This table obviates the variable of *state violence* as this is, by design of the matching algorithm, the variable that explains the selection mechanism of the cases in the sample.

C.3 Geographic Distribution of State Violence

Figure C.2 maps the locations of the municipalities affected by state violence from the Spanish security forces. The map shows that they are scattered throughout the territory but clustered around the four capital provinces, precisely the location of the Spanish police major headquarters.

Figure C.1: Map of State Violence in Catalonia



Note: The map shows the the areas of the Catalan region that suffered from violence by the Spanish police on the past October 1, 2017. Yellow circles show the location of the province capitals where the major Spanish police headquarters are located.

C.4 Interpolated Size of Mass Graves in Spain

Figure C.2 shows the interpolated size of mass graves (log scale) for all Spanish municipalities.

Figure C.2: Map of Interpolated Size of Mass Graves in Spain

C.5 Effects of Primes of Violence on Polarization. Full Tables.

C.5.1 Effects on Neutral Parties. Full table.

Table C.2: Estimates of the Effects of Collective Threat Messages on Voting for Neutral Parties

	DV: % Vote for Neutral Parties, 2017			
	ITT		*CACE*	
	(1)	(2)	(3)	(4)
Threat letter	-0.64^{*}	-0.54^{*}	-1.05^{*}	-0.90^{*}
	(0.39)	(0.33)	(0.66)	(0.56)
Placebo letter	-0.21		-0.33	
	(0.45)		(0.70)	
Omitted Group	No letter	Placebo or no letter	No letter	Placebo or no letter
Lagged DV: % Vote Neutral Parties, 2015	0.42^{***}	0.42^{***}	0.41^{***}	0.41^{***}
	(0.05)	(0.05)	(0.05)	(0.05)
Constant	1.45^{***}	1.35^{***}	1.55^{***}	1.43^{***}
	(0.53)	(0.49)	(0.58)	(0.51)
N Villages	140	140	140	140
R^2	0.35	0.35	0.31	0.31

Note: *p<0.1; **p<0.05; ***p<0.01. One-tailed tests.

C.5.2 Effects on Unionist Parties. Full Table.

Table C.3: Estimates of the Effects of Priming Collective Threat on Voting Behavior

	DV: % Vote for Unionist Parties, 2017			
	ITT		*CACE*	
	(1)	(2)	(3)	(4)
Threat letter	0.12	0.06	0.14	0.07
	(0.53)	(0.45)	(0.87)	(0.74)
Placebo letter	0.14		0.15	
	(0.61)		(0.93)	
Omitted Group	No letter	Placebo or no letter	No letter	Placebo or no letter
Lagged DV: % Vote Pro-Unionist, 2015	1.00***	1.00***	1.00***	1.00***
	(0.02)	(0.02)	(0.02)	(0.02)
Constant	3.53***	3.59***	3.56***	3.60***
	(0.59)	(0.54)	(0.57)	(0.53)
N Villages	140	140	140	140
R^2	0.93	0.93	0.93	0.93

Notes: *p<0.1; **p<0.05; ***p<0.01. One-tailed tests.

C.6 The Effects on Neutral and Unionist Parties against the Sharp Null

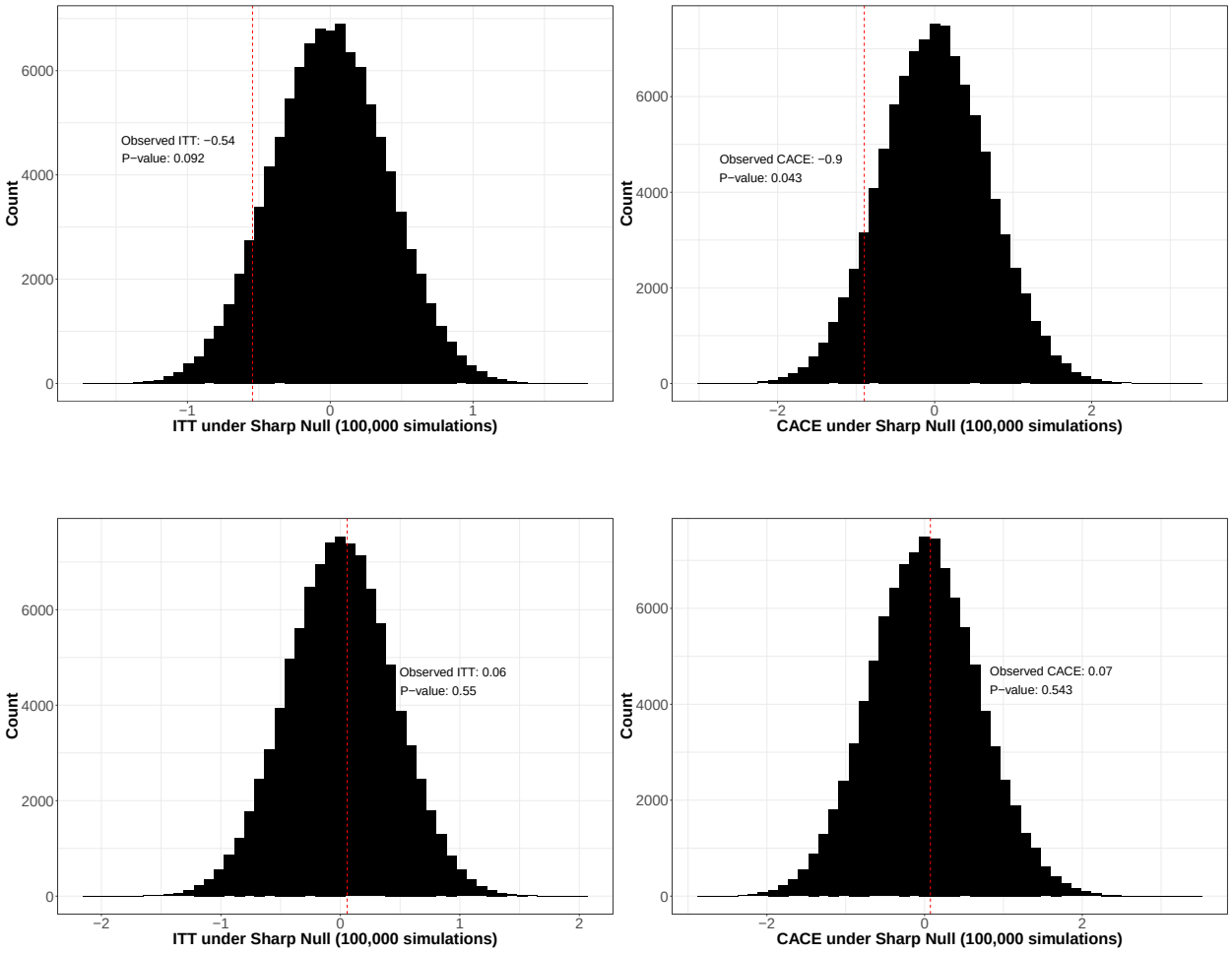
In the main body of the text, I assessed the likelihood that these differences in support for pro-independence parties would have arisen by chance by testing the sharp null hypothesis under the randomization inference framework. In this online Appendix, I also calculate the sharp null p-value under the hypothesis of no treatment effect for all observation with regards to the change in the vote for neutral and unionist parties, respectively.

For this, I follow the same procedure discussed in the main body of the text. First, we compute a complete schedule of potential outcomes. From this, I generate four randomization distributions by randomly sampling four times 100,000 possible vectors of treatment assignments. Figure C.3 shows the distribution of the simulated ITT and CACE effects under random assignment of the treatment on support for neutral parties (upper row) and unionist parties (bottom row) in these 100,000 possible assignments.

The upper left panel shows the distribution of 100,000 simulations of the ITT effect on neutral parties under a strict null of no effect. The dashed red line indicates the observed ITT effect of the experiment on neutral parties. The p-value indicates the percentage of simulated effects under random assignment that are greater than the observed treatment effect. Overall, we observe that only 9.2% of the simulated ITT effects on neutral parties are as large as the observed differences.

The upper right panel shows the distribution of 100,000 simulations of the CACE effect on neutral parties under a strict null of no effect. The estimated CACE is obtained from implementing the instrumental variable method discussed in the main text for each randomly generated treatment vector. The dashed red line indicate the observed CACE effect of the

Figure C.3: Distribution of Effects Under a Strict Null: Neutral and Unionist Parties



experiment on neutral parties. The p-value indicates the percentage of simulated effects under random assignment that are greater than the observed treatment effect. Overall, we observe that only 4.3% of the simulated CACE effects on neutral parties are as large as the observed differences. Therefore, the observed difference across treatment conditions are unlikely to have arisen by chance alone.

The bottom left panel shows the distribution of 100,000 simulations of the ITT effect on unionist parties under a strict null of no effect. The dashed red line indicates the observed

ITT of the experiment. Again, the p-value indicates the percentage of simulated effects under random assignment that are greater than the observed treatment effect. Overall, we observe that 55% of the simulated ITT effects are, at least, as large as the observed differences. Similarly, The bottom right panel shows the distribution of 100,000 simulations of the CACE effect on unionist parties under a strict null of no effect. The dashed red line indicates the observed CACE of the experiment. Again, the p-value indicates the percentage of simulated effects under random assignment that are greater than the observed treatment effect. Overall, we observe that about 54% of the simulated ITT effects are, at least, as large as the observed differences. Therefore, we can conclude that an effect as large as the observed ITT or CACE for unionist parties is likely to have arisen simply by chance.

C.6.1 Effects of Collective Threat Messages on Electoral Mobilization

A potential consideration is that a campaign of reminders of state violence might have mobilized voters to turn out in the elections. Hence, I evaluate the impact of the experimental treatments on electoral mobilization. Table C.4 shows the effects of experimentally inducing collective threat. The leaflet campaign leads, at most, to 0.03 percentage points decrease in turnout. Even when the estimates account for compliance, treatment letters lead, at most, to 0.11 percentage points decrease in turnout. None of the effects are statistically distinguishable from a null result.

Table C.4: Estimates of the Effects of Collective Threat Messages on Turnout

	DV: % Turnout, 2017			
	ITT		*CACE*	
	(1)	(2)	(3)	(4)
Threat letter	−0.03	0.00	−0.11	−0.02
	(0.49)	(0.44)	(0.86)	(0.73)
Placebo letter	−0.08		−0.18	
	(0.59)		(0.90)	
Omitted Group	No letter	Placebo or no letter	No letter	Placebo or no letter
Lagged DV: % Turnout, 2015	0.79***	0.79***	0.79***	0.79***
	(0.05)	(0.05)	(0.05)	(0.05)
Constant	20.07***	20.03***	20.04***	20.02***
	(3.75)	(3.72)	(3.76)	(3.75)
N Villages	140	140	140	140
R^2	0.69	0.69	0.69	0.69

Notes: *p<0.1; **p<0.05; ***p<0.01. One-tailed tests.

C.7 Balance of Historical Violence by Experimental Condition

Table C.5: Two-sample Kolmogorov-Smirnov tests

	D statistics	p-value
Treatment vs. Placebo	0.17	0.52
Treatment vs. Control	0.19	0.29
Placebo vs. Control	0.19	0.43
Treatment vs. Placebo + Control	0.15	0.40